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Les Océans et les Dangers Résultants de L'Utilisation de l'Energie Atomique

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Il y a quelques mois, s'est tenu à Paris, un colloque sur les dangers atomiques, colloque qui réunissait non seulement des Français, mais aussi certains spécialistes étrangers. A la suite de l'exposé et de la discussion d'un rapport dont j'avais été chargé sur les dangers que fera courir aux êtres aquatiques l'emploi de plus en plus intensifié des radioéléments, un voeu fut émis souhaitant qu'une réglementation internationale soit établie en ce qui concerne le dépôt éventuel des produits de déchets de l'industrie atomique dans les eaux marines.

Nous avons pensé que, s'il était un organisme qui devait prendre la charge de promouvoir un mouvement visant à assurer la protection des océans, c'était bien le Conseil International, et c'est pourquoi je me propose d'exposer brièvement comment, à mon sens, se présente le problème.

Tout d'abord, rappelons quelles sont les formes sous lesquelles les êtres marins peuvent être exposés aux dangers atomiques. Elles me semblent être essentiellement de trois ordres:—

a) les explosions atomiques;
b) l'utilisation des eaux dans les usines atomiques, eaux qui sont ensuite retournées aux rivières. Celles-ci, plus ou moins polluées, charrient les éléments radioactifs dans les eaux marines;
c) enfin, l'immersion, dans les fonds océaniques, de containers renfermant les déchets atomiques.

Voyons d'abord les explosions atomiques. Toutes les explosions atomiques à la surface du globe peuvent contaminer les eaux marines. Les explosions terrestres par écoulement des eaux douces polluées ou par retombée de nuages radioactifs, et nous savons les distances considérables auxquelles peuvent être transportés ces nuages radioactifs.

Mais ce sont évidemment les explosions marines, pour une même libération d'énergie, qui entraînent les dangers les plus grands de contamination pour le monde marin et nous en avons des exemples dans les explosions du Pacifique et dans les rapports qui ont suivi.

On sait l'histoire malheureuse des pêcheurs japonais du "Fukuryu Maru", la contamination des poissons dont certains migrent fort loin des lieux de l'explosion et la perte importante qui s'en est suivie pour l'économie japonaise. D'un point de vue général il y a eu contamination dangereuse du milieu marin. Je ne pense pas cependant insister sur les dangers de ces explosions atomiques pour les raisons suivantes.

D'une part, ces dangers sont évidents et ne sont donc contestés par personne. D'autre part, ces explosions atomiques sont condamnées par la majorité des habitants de cette planète, non pas du fait des dangers qu'elles représentent pour les animaux marins, mais du fait des dangers qu'elles représentent pour nous-mêmes directement. Certes, les océanographes doivent, je pense, joindre leurs conseils pressants de sagesse à ceux des savants des autres disciplines, mais ils ne seront écoutés qu'autant qu'ils auront convaincu les Maîtres du Jour que l'intégrité de la vie humaine est liée à celle de la vie marine. Enfin, si les chefs responsables décident, en dépit de la réprobation qu'elles suscitent, de poursuivre ces expériences atomiques, la considération des dangers encourus par la vie marine pèsera peu en regard des dangers dont est directement menacée l'humanité et ce sont évidemment les régions les plus éloignées des agglomérations humaines, et qui se trouvent à la disposition des divers états, c'est-à-dire souvent les océans, qui seront choisis pour ces redoutables expériences.

Voilà pourquoi je n'insisterai pas sur cette première source de pollution.

La seconde source de pollution que j'ai citée est représentée par les eaux utilisées par les usines atomiques. Les réacteurs sont, dans certaines usines, refroidis avec de grands volumes d'une eau qui devient radioactive. Cette eau et certains autres résidus liquides radioactifs provenant de la fabrication sont déversés dans les fleuves ou même, dans certains cas, par pipe line dans la mer. Une partie de cette radioactivité peut être fixée par la vase et se déposer dans les fonds fluviaux ou sous-marins, mais il subsiste toujours une certaine radioactivité en solution.

Quelle est la radioactivité des fleuves dont les eaux ont été utilisées par les usines atomiques? MORGAN, d'après HIGGINS, donne comme activité maximum trouvée dans la Clinch River qui reçoit certains déchets de Oak Ridge la valeur de $3{\cdot}7 \cdot 10^{-7}$ microcuries par cm^3., approximativement équivalent à 1/100,000 r. par jour. COOPEY a étudié en 1953 la radioactivité des eaux de la rivière Columbia qui sont utilisées dans l'usine atomique d'Hanford. Elle était en moyenne de $1{\cdot}6 \cdot 10^{-6}$ μc. par cm^3. C'est évidemment une radioactivité très faible incapable par son seul rayonnement externe d'apporter quelque désordre actuel dans la vie aquatique. De plus, les auteurs qui ont sous-estimé les dangers de la contamination atomique ont présenté le calcul suivant. La concentration maximum de radium autorisée dans l'eau de boisson a été fixée à $4 \cdot 10^{-8}$ μc./cm^3., ce qui équivaut à $4 \cdot 10^{-11}$ g. de radium par litre, soit 40 g. par Km^3. Or l'eau de mer contient

environ $1 \cdot 10^{-13}$ g. par litre. La quantité de radium contenue dans 1 Km^3. d'eau de mer est donc 10^{-13} g. 10^{12} litres = 0·1 g. Pour élever la concentration au niveau permis pour l'eau de boisson, il faudrait donc ajouter à l'océan 39·9 g. par Km^3.

Etant donné le volume considérable des océans et des mers du globe qui a été estimé à plus d'un milliard de kilomètres cubes ($1{\cdot}37 \cdot 10^9$ Km^3.), on ne peut actuellement envisager le laps de temps au bout duquel le dépôt des déchets radioactifs aura conduit l'ensemble des océans à ce niveau de radioactivité.

Sans doute, mais ce calcul ne tient pas compte des réalités océanographiques. Nous savons parfaitement qu'à l'arrivée d'un fleuve pollué dans la mer, il se produira une concentration régionale, durable et probablement croissante dans le temps, d'éléments radioactifs. Les vases, les microorganismes charriés par le fleuve et qui auront, en eau douce même, fixé les radioéléments, viendront s'accumuler peu à peu dans cette zone radioactive, l'étendront, et au sein de ce biotope, les êtres vivants, même s'ils n'y restent que quelques heures ou quelques jours, concentreront en eux-mêmes cette radioactivité, car c'est là une notion générale et capitale s'appliquant aussi bien aux isotopes stables qu'aux isotopes instables que celle-ci dont le professeur SVERDRUP a donné des exemples particulièrement frappants dans son magistral ouvrage: à savoir que certains êtres vivants peuvent concentrer de façon étonnante certains éléments chimiques. Par exemple, la concentration est de 4,300 pour le cuivre et de 6,900 pour le fluor chez un mollusque marin, de 13,000 pour le silicium chez un copépode et de 2,560,000 pour le phosphore chez un poisson. Les isotopes radioactifs ne font pas exception; ils ne se comportent pas autrement que les isotopes stables. Il faut simplement souligner que, pour un animal vivant dans une eau donnée contenant un isotope stable et un isotope radioactif, du potassium-39 et du potassium-42, par exemple, la proportion des atomes radioactifs parmi les atomes stables sera la même dans les tissus qui les ont absorbés que dans la nature. Si nous appelons dilution isotopique, cette dilution des atomes radioactifs dans les atomes stables, la dilution isotopique diminue évidemment le danger de certains corps radioactifs éliminés dans les eaux. Et ce facteur doit jouer surtout dans les eaux marines, plus fortement minéralisées que les eaux douces pour certains éléments, *mais seulement pour certains éléments*, car dans les produits de fission qui souillent les eaux marines, nous trouvons des radioisotopes de nombreux éléments rares, des éléments qui ne se trouvent qu'à une concentration infime dans l'eau de mer et pour les quels la dilution isotopique ne joue que fort peu. C'est ainsi que j'attirerai l'attention sur un radioélément qui se trouve en proportion importante dans les produits de fission, le strontium-90 qui présente une demi-vie d'un quart de siècle, qui se fixe intensément dans les os et est excrété très lentement, si bien que même dans des eaux très peu chargées en cet élément, la radioactivité peut être notable dans les êtres vivants et par sa permanence, entraîner à longue échéance des conséquences funestes.

Voyons quelques exemples des constatations d'une concentration des radioéléments par les êtres marins.

En ce qui concerne la mesure de la radioactivité globale, citons les données de MORGAN pour la Clinch River qui reçoit certains déchets d'Oak Ridge. Pour les eaux de cette rivière, MORGAN trouve que le plancton présente une radioactivité qui est en moyenne 10,000 fois plus grande que celle de l'eau ambiante. Dans la Columbia River, HIGGINS cite une concentration de la radioactivité par les algues qui atteint près de 1000 fois celle de l'eau.

Certains éléments ont été étudiés isolément. C'est ainsi que SPOONER, au laboratoire de Plymouth, a étudié les fixations des strontium-89 et -90 par les algues et a trouvé que *Fucus serratus* concentre environ 40 fois le strontium de l'eau de mer, *Fucus vesiculosus* 30 fois, *Ascophyllum nodosum* 20 fois. La concentration de l'iode par les algues est une donnée classique et rappelons, d'une part, que les radioiodes sont des éléments qu'on trouve dans les produits de fission, d'autre part, que dans certaines régions, l'homme consomme directement les algues, dans d'autres plus étendues, il les utilise pour l'alimentation du bétail, dans la pharmacopée, dans l'industrie. La radioactivité des algues peut donc avoir des conséquences imprévisibles dans les domaines les plus variés.

En ce qui concerne les invertébrés, je citerai les expériences de GORBMANN, CLEMENTS, et O'BRIEN qui ont montré que de petits mollusques bivalves — d'eau douce cette fois — ont concentré en moins de 3 jours près de 100 fois le radioiode contenu dans l'eau. Et il faut souligner que si l'iode-131 présente une période relativement courte (8 jours), l'iode-129 présente une période de 20 millions d'années.

En ce qui concerne les poissons, PRESSER a réalisé une expérience particulièrement intéressante puisqu'il immerge simplement des poissons rouges dans des eaux contenant des produits de fission de l'uranium, du sodium, du strontium, du baryum, et des terres rares radioactifs. C'est donc une expérience très intéressante puisqu'elle se rapproche des conditions normales dans lesquelles se trouve un poisson vivant dans les eaux provenant d'une pollution atomique. Le rapport de concentration de radioactivité dans le poisson à la radioactivité de l'eau s'accroît avec le temps et peut arriver à être égal à 100, ce rapport étant indépendant de la dose dans les larges limites étudiées par ces auteurs (0·01 μc. à 1·0 μc. par cc.). Il importe de préciser qu'il existe de grosses différences spécifiques dans la captation d'un isotope donné et, pour une espèce déterminée, de grandes différences selon l'état physiologique du sujet, et les conditions extrinsèques, température des eaux par exemple. En voici quelques exemples. FRETTER (1953) a étudié la fixation du radiostrontium dans un certain nombre de mollusques et de polychètes et constaté qu'il se localise différemment dans l'organisme suivant l'espèce considérée. BROOKS et CHAMBERS (1954) ont étudié la pénétration du phosphore-32 sous forme d'orthophosphates dans les œufs de deux oursins, *Strongylocentrotus purpuratus* et de *S. franciscanus*, et d'un Echiuridé, *Urechis caupo*.

Chez les oursins, dans les 10 à 15 minutes qui suivent la fécondation, la vitesse de captation du phosphore-32 est identique à celle des œufs non fécondés qui est très lente. Elle s'accroît ensuite pour atteindre son maximum de 50 à 90 minutes après la fécondation et reste constante pendant les 3 premières divisions. L'absorption du radiophosphore est augmentée de 100

fois quand on passe de l'œuf vierge à l'œuf fécondé. La cinétique du phénomène est très différente pour un autre œuf d'invertébré, *Urechis caupo*. Ici, l'accélération de la fixation après fécondation est beaucoup plus lente à se produire. Elle ne commence à se manifester qu'à la seconde division de l'œuf et, lors de la quatrième division, la vitesse de fixation n'est encore que 7 fois celle de l'œuf non fécondé, mais, à ce moment, la pente de la courbe augmente toujours, le maximum n'est pas encore atteint.

D'autre part, RUGH et GLUGSTON (1955) ont montré que les œufs d'artémia sont plus radiosensibles à l'état humide qu'à l'état sec.

Voici donc des exemples, d'une part de différences spécifiques importantes de captation de radioisotopes selon l'espèce et d'autre part selon l'état physiologique (ovule vierge ou fécondé, œufs hydratés ou non).

Soulignons aussi que l'état sous lequel le radioélément pénètre dans l'organisme est très important. RODINA (1954) a montré que le coefficient d'accumulation du radiophosphore par des invertébrés aquatiques est beaucoup plus élevé lorsque celui-ci est ingéré sous forme d'aliments radioactifs que lorsqu'il est simplement en solution dans l'eau.

Chez les poissons, les premiers résultats obtenus, surtout avec l'iode-131, montrent qu'il existe également de fortes différences spécifiques, des fluctuations suivant les conditions du milieu extérieur, notamment la température, suivant l'état nutritionnel et l'état physiologique de l'animal. En voici quelques exemples.

Quand, en 1948 et 1949, les biologistes procédèrent dans le secteur de Bikini à des mesures de radioactivité de leurs captures, ils constatèrent que les espèces de poissons herbivores, comme les poissons perroquets, fixaient plus de radioactivité que les espèces carnivores. La différence était important puisqu'elle était dans le rapport de 1 à 10.

A quoi est-elle due?

Bien que les différences spécifiques soient très grandes et qu'en l'absence de précisions sur les espèces consommées par les poissons, il soit difficile de conclure, il ne semble toutefois pas d'après les valeurs données par les prospecteurs des atolls que les algues aient en moyenne fixé beaucoup plus de radioactivité que les animaux.

Enfin, à mon laboratoire nous avons constaté que le jeûne entraîne une diminution importante de la captation de l'iode-131 chez la truite (M. FONTAINE et Y. A. FONTAINE) et que les variations de température de l'eau modifient cette captation (M. OLIVEREAU, M. FONTAINE, M. M. BARADUC, et Y. A. FONTAINE). Récemment KRUMHOLZ signale que deux espèces de poissons d'eau douce concentrent le radiostrontium de 20,000 à 30,000 fois, les *Pomoxis* sensiblement plus que les *Lepomis*.

La question se pose maintenant de savoir si les radioactivités décelées dans le milieu marin à la suite de ces pollutions sont susceptibles d'exercer des effets sur la vie marine. Si nous envisageons les expériences destinées à nous éclairer sur la nocivité de la radioactivité sur les animaux aquatiques, nous nous trouvons principalement en présence d'expériences qui ont consisté à irradier des animaux aux rayons X, correspondant aux rayons γ de la pollution atomique. Mais ce sont là des conditions nettement différentes de celles réalisées dans les eaux marines par les produits de fission. En effet,

les produits de fission émettent plus de rayons β que de rayons γ, et, si les rayons β pénètrent beaucoup moins à l'intérieur des tissus que les rayons γ, ils peuvent, à la surface des téguments, produire des lésions très dangereuses, mais surtout, s'ils sont ingérés, ils entraînent des perturbations très graves du métabolisme cellulaire qui peuvent aboutir à la destruction complète de certains tissus et naturellement à la mort de l'animal. Or, ces noyaux émetteurs d'électrons (radioactivité β) pénètrent dans le corps des animaux marins. Les poissons, par exemple, peuvent les absorber soit simplement par absorption d'eau de mer qu'ils boivent abondamment pour compenser les pertes osmotiques d'eau dues à l'hypertonie du milieu externe sur le milieu intérieur, soit avec leurs aliments, avec le plancton notamment qui, comme nous l'avons vu précedemment, peut les concentrer de l'ordre de 10,000 fois.

S'il faut des radioactivités relativement importantes pour tuer rapidement les êtres marins, les radioactivités faibles qui seules résultent jusqu'ici des pollutions auxquelles je fais allusion, peuvent agir à long terme sur les capacités reproductrices des êtres vivants et sur la qualité de la descendance, par le phénomène des mutations. Elles agissent sur toutes les formes de vie, des virus à l'homme, et les mutations portant sur les bactéries par exemple, peuvent agir indirectement sur les métazoaires d'une façon aussi catastrophique que par une action directe et aussi plus rapidement puisqu'ils peuvent faire apparaître des bactéries pathogènes de la plus grande nocivité. De plus, il n'existe pas de seuil pour ces actions nocives des irradiations; c'est-à-dire que de très faibles radioactivités additionnent à la longue leurs effets. L'expérience que nous pouvons avoir de la nocivité des produits de fission répandus dans les eaux est ridiculement brève et pourtant déjà des observateurs tirent la sonnette d'alarme en ce qui concerne la faune d'eau douce.

Au cours d'une vaste enquête écologique dirigée par la Tennessee Valley Authority pour la Commission de l'Energie Atomique des Etats-Unis, des remarques particulièrement intéressantes ont été faites dans le lac de White Oak, lac de retenue du White Oak Creek. Les observateurs ont suivi les modifications écologiques de ces eaux pendant une durée encore très faible — 3 années.

Ces eaux ne sont que très légèrement polluées, car tous les déchets contenant des produits de fission à une concentration relativement élevée sont ensevelis sous la terre et pourtant, en dépit de ceci, en dépit du très court laps de temps d'observation, KRUMHOLZ observe une augmentation progressive de la radioactivité des eaux, des modifications dans la faune qu'il a étudiée. Les taux de croissance des poissons ont été ralentis, la longévité diminuée. Deux espèces de poissons avaient même pratiquement disparu (*Pomoxis annularis* et *Moxostoma aureolum*).

Je citerai aussi, bien que relevant d'une contamination par explosion atomique, les travaux des Japonais, FUJITA, MATSUBARA, HIROKAWA, et ARAKI, qui ont récemment publié que dans le district d'Hiroshima, les jeunes huîtres, *Ostrea gigas*, souffrent d'une mortalité inhabituellement haute. Et l'étude histologique montre la destruction par de nombreuses

cellules envahissant les tissus parenchymateux de divers organes et principalement de l'ovaire.

Je dois maintenant dire quelques mots du troisième type de pollution que j'ai envisagé au début de cet exposé. Le dépôt dans les eaux profondes des résidus introduits dans des récipients de diverses natures. Ce procédé présente l'avantage de faire disparaître sans retour à nos yeux des produits bien encombrants, mais il n'est pas sans péril pour l'avenir car il est fort probable que la durée de vie du récipient les contenant sera très inférieure à la durée de la radioactivité. Songeons que l'iode-129 présente une période de 20 millions d'années, que le césium-135 présente une période de 3 millions, le zirconium-93 une période d'1 million d'années, et que ces corps seuls représentent plus de 10% des produits de fission des éléments lourds, uranium-235 et plutonium-239. Et rappelons qu'au terme d'une période, la radioactivité existe encore, étant seulement moitié de ce qu'elle est initialement. Or, qui pourrait nous garantir que les containers résisteront plus d'1 million d'années à l'érosion des eaux marines. Sans doute, les auteurs du fascicule 58 du Departement of Commerce des U.S.A. conseillent-ils d'immerger ces récipients contenant les déchets atomiques au-dessus des fonds de vase de 2,000 mètres, espèrant que les containers voudront bien s'enforcer suffisamment et, confiants dans la lenteur des échanges entre ces eaux de fond et les eaux de surface, le jour où les parois des containers livreront leur contenu. Mais, si la lenteur de ces échanges est évidente à l'échelle de notre temps, elle l'est beaucoup moins à l'échelle de temps à laquelle je viens de faire allusion. D'ailleurs, dans bien des cas, les êtres vivants ne se chargeront-ils pas d'assurer le transfer des produits de fission? On sait, par exemple, qu'on trouve dans les estomacs de thon des représentants de certaines espèces d'invertébrés (céphalopodes par exemple) qui ont été également capturés en eau profonde.

Je pense donc que, si ce procédé décharge notre génération d'un lourd souci, il est très dangereux pour l'avenir. Il importe, de repenser la notion de temps; "l'homme est un infirme", disait JEAN COCTEAU dans son récent discours de réception à l'Académie Française, "un infirme prisonnier de ses dimensions". Sans doute, mais l'infirme doit chercher à s'évader de sa prison. Nous devons avoir conscience que notre génération n'est qu'un maillon de la très longue chaine qui représente l'évolution de notre espèce et qu'il est un devoir pour chacun de nous de ne pas laisser s'établir à la surface du globe des conditions risquant de compromettre la solidité des maillons futurs. Sans doute la solidité de maillon que représente notre génération n'est-elle pas gravement menacée par l'utilisation pacifique de l'energie atomique, mais je suis persuadé que cette utilisation, telle qu'elle est actuellement conçue, comporte un danger certain pour la solidité des futurs maillons. Et je pense que c'est un devoir pour le Conseil de se préoccuper de ce grave problème dans le domaine qui lui est propre, c'est-à-dire en préservant les océans d'une contamination qui, aujourd'hui, apparaît très limitée, mais qui, dans quelques décades ou dans quelques siècles, peut se révéler tragique et absolument irrémédiable.

Résumé

The use of atomic energy is growing rapidly, and oceanic life is thereby exposed to three different types of agression, namely, (a) atomic explosions, (b) the return of water used in atomic factories to the ocean, either directly or by way of the rivers, and (c) the deposition in the oceans of containers holding atomic wastes.

(a) It is hardly necessary to refer to the first type of agression because its dangers are evident and have unfortunately been confirmed by a well known experience.

(b) The dangers arising from the discharge of water polluted by atomic factories into the rivers or directly into the oceans have been underestimated, and for this reason are especially emphasized. Attention is drawn to the following points: —

The atomic wastes do not immediately spread throughout the oceans; they form local concentrations. In such an infected region, the living creatures rapidly concentrate enormous quantities of fission products, the more especially as most of these products belong to rare elements on which isotopic dilution has very little action. On the whole, the fission products emit more β- than γ-rays and consequently their harmfulness cannot be inferred from the results obtained by the irradiation of aquatic animals with X-rays, the nature of which is the same as that of γ-rays. All we can say is that these products penetrate into the organism (together with water or food) and that the β-rays then have a baneful effect, about which X-ray irradiation experiments tell us nothing.

(c) The deposition of containers filled with atomic wastes on the bottom of the oceans has been found to be a convenient way of getting rid of products likely to be troublesome for a long while. The process is not without future danger, however, for it is very probable that a container will not last as long as the radio-activity.

The author urges the Council to direct its attention to this very important problem and to strive to protect the oceans from a pollution which, though it may seem insignificant today, may in course of time become tragic and absolutely irreparable.

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A Note on Synoptic Hydrographic Charts

By

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THE charts discussed in the present paper, which was read at the meeting of the International Council for the Exploration of the Sea in October 1952, cover the period June 1950 to May 1952, thus just two full years. They show, for each month, the salinity and temperature distributions, the currents, and the winds; they are accompanied by a brief descriptive note. These charts came into being as a result of a recommendation made by the Hydrographical-Biological Subcommittee of the Hydrographic Committee at the meeting of the International Council in October 1950. As there was some doubt about the possibility of their production in Copenhagen at that time, the Fisheries Laboratory, Lowestoft, undertook to prepare and publish them for a trial period.

The requirement stated was the production of monthly charts showing the distribution of surface temperature and salinity, together with the wind conditions; the charts were to be published as soon after the collection of the data as possible, and they were to cover "the North Sea and adjacent waters". The intention of the meeting, if I interpret it correctly, was, therefore, the presentation of information to fishery biologists early and regularly, and in such a form as to provide a picture of the fields of certain properties of the sea water obtaining over the general region of the fishing grounds round the British Islands in as much detail as possible. This would be in contrast to irregularly timed information relating to a variety of particular localities of limited extent.

It has probably been noticed that charts of phosphate distribution were also mentioned in the subcommittee's recommendations, and during the discussions desires were expressed for charts relating to subsurface waters, in addition to those of the surface. I am obliged to point out, however, that the number of surface phosphate observations and the number of subsurface data of any kind are altogether insufficient to permit charts to be prepared, except for relatively small areas and then only spasmodically.

The data at present available for these fields are quite inadequate for any regular synoptic survey. For such a survey there must be a basic network of observation positions, sufficient to indicate the general pattern of the distribution, the details of which are filled in as opportunity arises. But for subsurface information, no wide-scale network exists, and the information we possess relates mostly to limited localities. If such charts are, or should become, an essential need in fishery investigation, it is necessary to arrange, firstly, for the regular collection of the data and, secondly, for their subsequent treatment and collation. These steps are obvious, but I think that the extent of the organization required to achieve them is sometimes a limiting factor and must be carefully considered before such a plan can be operated. Except in a few particular cases (e. g., lightships, Ocean Weather Ships) subsurface observations at present require special ships and observers. It is doubtful whether there are at present sufficient numbers of such ships and staff to employ on the collection of the observations required for synoptic charts of subsurface conditions, like those which have been produced for the surface. As regards the second point, the treatment of the data, I think it is safe to say that we, as an international body, are not at present in a position to handle so great a number of observations as would be collected under such a programme.

Fifty years ago, when the Council was first established, a network of observation positions in the North Sea and English Channel was decided upon and quarterly cruises initiated. These cruises provided the data for the construction of quarterly synoptic[1]) charts, from which we learnt the broad outlines of the distributions of certain properties of the sea water, principally salinity and temperature. Gradually, however, this programme fell into abeyance, and ceased altogether on the outbreak of war in 1914. In case it should be thought that I am suggesting the revival of these cruises, may I say at once that, in my opinion, they should not be revived? They have served their purpose and their time of usefulness has passed. On the contrary, I believe we should try to step forward, not backward, and we may do this by developing and improving apparatus and techniques for obtaining observations, especially subsurface, which will operate automatically and could be used from ships other than research ships. By these means we should be able to acquire regularly and systematically collected information about those further characteristics of the sea water of which we desire to know more, but for which we lack the data at present. Development is already taking this direction, as witness the construction of salinity recorders, the improvement and development of recording thermometers (e. g., the bathythermograph), the employment of hitherto unused principles in measuring currents, and so on. These new devices will eventually provide us with large numbers of observations, perhaps of rather less

[1]) Such charts are sometimes referred to nowadays as "quasi-synoptic". The qualification is introduced, I presume, because the data used were not collected simultaneously. According to the Concise Oxford Dictionary (1931), the meaning of synoptic is simply "affording a conspectus or general survey"; it appears therefore that the charts may be described as synoptic quite fairly and properly.

accuracy than those obtained with the traditional forms of apparatus, but much closer together in space and time than has been possible hitherto. With the aid of such instruments we may hopefully look forward to a very considerable widening of our knowledge and understanding of the conditions and processes prevailing in the sea. But in the meantime we may try to make the best of such observations as we can obtain now.

At the same time as the quarterly surveys were started, to supplement the observations obtained from them, the Council also promoted a programme for the regular collection of temperature and salinity observations from merchant ships and other non-specialized vessels. These observations were perforce confined mainly to the surface because no suitable means were available of extending them into the depths. This collection has been continued, with some interruption due to wars, up to the present time, and a quite impressive material of observation has been built up. The data have been published, though not until some considerable interval has elapsed after collection, and they have been studied from time to time in various ways and for different regions and periods. They form the bulk of the only data available to us at present which seem likely to be suitable for synoptical treatment so as to furnish a means of appreciating the changing fields of both salinity and temperature. That these data at any rate can be so treated is demonstrated by the production of the present series of charts.

The charts produced, therefore, are limited, for salinity and temperature, to sea surface conditions. In addition they cover the winds, for which information is available in several forms, and also the currents. Regarding these, I should mention that at about the time we began work on these charts the measurement of currents, which ceased during the war, was started afresh in Dover Straits. This kind of activity has since become more widespread, and there is now a network of observation stations providing a fair cover for the southernmost parts of the North Sea. Although the current was not one of the characteristics for which information was originally asked, I decided to include it in the series of charts, as it seemed the natural complement of the information on the distributions of salinity and temperature, and of the wind. The measurements at the "Varne", "Galloper", "Smith's Knoll", and "Dowsing" lightships were carried out for the British Admiralty and the National Institute of Oceanography, and I have to thank Dr. CARRUTHERS for kindly providing me with the data. I am also greatly obliged to Dr. GROEN, of the Royal Netherlands Meteorological Institute, for the data from the "Goeree", "Texel", and "Terschellingerbank" lightships and to Dr. BÖHNECKE, of the German Hydrographic Institute, for those from "S. 2" lightship. The currents relate to a belt of water about 1 m. in thickness centred at about 2 m. depth.

The wind data for the shore positions in the British Islands and Weather Ship Stations "I" and "J" were obtained from observations made four times a day and recorded in the Daily Weather Reports of the British Air Ministry. The observations used for the continental shore stations were extracted from certain meteorological charts, by kind permission of the Air Ministry. The data for the three positions at sea in the North Sea were calculated from

the pressure gradients by Dr. G. DIETRICH, of the German Hydrographic Institute, and were kindly supplied to me through Dr. CARRUTHERS.

In addition to those collected by ourselves, salinity and temperature data were supplied to me by Dr. EGGVIN (Bergen), Dr. KULLENBERG (Göteborg), Mr. THOMSEN (København), Dr. BÖHNECKE (Hamburg), Dr. GROEN (De Bilt), Dr. LELOUP (Ostende), the late Dr. LE GALL (Paris), Dr. WENT (Dublin), Dr. TAIT (Aberdeen), Professor PROUDMAN (Liverpool), and Mr. RUSSELL (Plymouth). For a short period, March to June 1951, I also received temperature data from Commander C. E. N. FRANKCOM, of the Marine Division of the British Meteorological Office. This extensive material needed, however, rather more preliminary elaboration than we felt able to deal with, and I was therefore obliged, with great regret, to abstain from using these data. I feel sure, nevertheless, that this source of data is still open to us and could be used by making suitable arrangements if it is felt to be necessary.

To all these gentlemen, I should like to express my sincere thanks for their kindness and trouble in preparing and forwarding the data to me so regularly and so soon after collection. Without their most generous help and co-operation, it would not have been possible to produce the charts in the way that they have been.

At first the charts were distributed about three months after the month to which they refer. It was found, however, that occasionally important data were unavoidably lost by this procedure, and later on the interval between collection of the observations and issue of the charts was lengthened to four months. By so doing, we found that very few data were received too late for inclusion in the charts.

The wind and current data are presented in the form of approximately 10-day averages, as this seemed the most convenient. It was not thought practicable to give the information for shorter periods, nor necessary to make the periods longer.

The temperature and salinity charts are intended to refer to the middle of the month (this choice of date is quite arbitrary, it may be mentioned, and similar charts may be drawn for any other date). This is achieved by using the averages of suitably spaced values where possible and, in the case of temperature, by adjusting them according to the prevalent trend. This, it is believed, can be done satisfactorily with temperature, but not with salinity. Broadly speaking, the annual course of temperature is more regular than that of salinity in these waters and the annual fluctuation predominates over other fluctuations which arise from various causes. The seasonal trend of temperature characterizing a particular locality can, I think, usually be estimated reasonably well, particularly when the rise or fall is rapid, though, of course, near the minimum or maximum of the seasonal curve the estimate is less reliable. At these seasons, too, the average values are often not quite so good an estimate of the temperature at mid-month as at times when the gradient of the annual trend is steep. I believe that the charts are a reasonably accurate portrayal of the temperature conditions at mid-month, and I judge the effectiveness of the procedure I have outlined for achieving this from

the fact that when the method is applied to what appears at first sight to be a more or less confused mass of observations for different dates and from the several sources, the data fall into coherent patterns and isotherms can be readily drawn.

With salinity the case is rather different. The annual fluctuation in salinity is, at any rate for the offshore waters, very small in relation to other fluctuations, and is therefore much more difficult to estimate in circumstances such as the present. On the other hand, the broad changes that occur in salinity appear to take place rather slowly and often occupy periods longer than a month. We find in this case that however varied may be the sources and dates of the observations in any month, they rarely appear so confused and difficult to interpret as a collection of temperature observations may do before adjustment, and it is usually possible to trace the coherence between them. By the exercise of a little judgement — for instance, by mentally weighting average values higher than single observations — I have very rarely found any considerable difficulty in plotting the courses of the isohalines reasonably satisfactorily — always provided, of course, that there are sufficient observations. This is not everywhere and at all times the case and is an insuperable limitation on the precise representation of the field we wish to survey whatever methods for adjustment we may adopt.

The choice of the area to be covered by the charts is fairly wide. In deciding what sea regions to include, I was guided by the number and distribution of the observations available, by the size of charts on which the details of the fields they were intended to portray could be drawn and shown reasonably clearly, and by the size of paper that was convenient in reproduction and handling. Naturally, some compromise between these different considerations had to be effected. The charts as they are include most, but not all, of the regions which might properly be described as adjacent to the North Sea and for which data are available. In particular, the Baltic, for instance, is excluded, and the narrow waters connecting the Baltic and North Sea are not covered. To provide a useful representation of the fields in these narrow waters would involve using too large a scale for the other sea regions. If charts for these areas are required, it would probably be advisable to produce them separately rather than combine them with the larger regions. On the other hand, although the data in the eastern Atlantic are rather too few to enable isolines, in particular isohalines, to be drawn with certainty, this area has been included in the charts, as it was thought that representation of conditions in these waters was of primary importance to the appreciation of conditions in the North Sea. There is no reason, of course, why future charts should cover precisely the same area as the present ones; indeed, these remarks are in no way intended to suggest that there is no room for improvement in this or in other respects.

The Notes which have accompanied the monthly sets of charts have been intentionally confined mainly to descriptions of anomalies and of salient features of the general fields; almost no attempt has been made to interpret the fields in terms of water movements. It has sometimes been the practice in the past to employ the courses of the isohalines, particularly, as a basis for the deduction of the direction of the currents. That this procedure is

not without justification is demonstrated by Dr. BÖHNECKE's celebrated work (1) on the current system of the North Sea, as inferred from salinity distribution. Dr. CARRUTHERS and I (2) have also shown that currents directly measured by meter fit remarkably well into the current pattern deducible from salinity distribution. Nevertheless, I feel that at present the uncertainties involved in this procedure are too numerous to make its use generally advisable. There is, moreover, very considerable danger of being misled by interpreting changes in the value of salinity as indicative of major water movements. A rise in salinity in the North Sea, for example, even over a wide area, does not necessarily result from an increased inflow from the Atlantic, either at an increased rate or in a greater volume, nor indeed from an influx of any water with a higher salinity. Similarly, a fall in salinity does not necessarily indicate a diminished Atlantic inflow. As regards the value of salinity and the speed of the current, for instance, Dr. WYRTKI (3) has studied the question of the relationship between these two elements in Dover Straits. He concludes that they appear to be quite unconnected; a high salinity does not indicate a strong north-easterly current, nor a low salinity a weak or reversed current. At the same time, however, Dr. WYRTKI also shows that it is sometimes possible to estimate the water movement over a fairly short period, such as a month, by observing the advance of a particular isohaline; he gives two examples from the English Channel, in which the rate of advance of isohalines in the eastern part of the Channel agrees well with currents measured at the "Varne" lightship. But I feel that even this method must be employed with caution; it may not be so readily applicable, for instance, to the extremity of the salt tongue stretching north-eastwards through the Southern Bight. While one of the factors governing the salinity of a region such as the North Sea is indeed the salinity of the water entering from the ocean, the local salinity is modified by rainfall and run-off from the land, by evaporation, and by the effects of currents and mixing, as Dr. BOWDEN (4) points out in his study of processes affecting salinity in the Irish Sea. He shows that the salinity variations of inshore waters reflect the variations in run-off, but in offshore waters the difference between precipitation and evaporation is the more decisive factor. Moreover, he points out the great importance of the part played in determining salinity distribution by mixing, not only laterally to the axis of the current, but also longitudinally. From recent investigations in the Southern Bight, my colleague, Mr. VAUX, (5) has reached the conclusion that some of the increases in salinity which have been observed there may be very largely, if not entirely, accounted for by processes of evaporation, especially in winter, the season when they are most effective.

The processes controlling the temperature of the water can be similarly separated and studied, and provided the several factors are borne in mind and some assessment of their relative effects can be made, it may perhaps become possible to employ synoptic charts of both temperature and salinity fields for following the movements of the major water masses in regions such as the North Sea. I may say that we have made some preliminary attempts to do this at Lowestoft, but have not been able to spend sufficient time on the work to produce results worth mentioning at present. I may

perhaps also mention at this point that we have tried to keep in mind the fact that the production of the charts should not be thought of as an end in itself, as the ultimate goal, at any rate from the hydrographer's point of view. The charts are more properly to be regarded as a stage in the working up of the data, for the purposes of further study. In meteorology synoptic charts form the basis for forecasting, and hydrographical charts may well come to serve a similar purpose. Indeed, to some extent they can already be used in this way. For instance, we may learn from the present series of charts that while the pattern of temperature distribution changes markedly from month to month in the course of a year, the patterns in corresponding months of different years are very alike. That is to say, the average chart for a month over a number of years is a fair forecast of what the temperature is likely to be in that month in any particular year.

With salinity, however, the position is rather different. The different months are not immediately distinguishable by characteristic salinity distributions, and the general annual mean chart would provide almost as good a forecast for a particular month as the general monthly mean chart. With salinity, it is the local and aperiodic changes which are of primary importance.

In this connexion it may be worth while to mention that we do not possess average monthly charts of salinity and temperature which cover the whole of the region included in the present charts[1]), and that the estimation of anomalies in a particular month involves reference to several publications. We are worse off in this respect for salinity than we are for temperature.

From the biological point of view, I hope — and believe — that the charts, in spite of their limitations, can serve an immediate purpose. So far as I know, there has never before been an opportunity to watch the changing patterns of salinity and temperature from month to month over so wide an area. The charts enable an appreciation to be made, not only of the general nature of these fields, but also of the type and order of magnitude of the changes that occur in them. The fact to which I have already referred — that the temperature distribution in a particular month is characteristic for that month — is an example of what I mean. This conception can of course be reached in other ways than by examination of the charts, but the charts bring out this feature particularly clearly, and show that it applies to the region as a whole.

Furthermore, the rapid presentation of the information is entirely new. It is hoped that the shortness of the time elapsing between the collection of the observations and their availability in chart form may lead to the recognition of important features or changes soon enough for them to be followed up by further investigations. Also, when local intensive surveys have been made, the synoptic charts may serve to relate local conditions to the field as a whole. This is sometimes of considerable importance to the proper appreciation of local conditions.

Yet another way in which the charts may serve a useful purpose for the

[1]) At the meeting of the Council in October 1954, it was decided that such charts should be prepared, and work on them has begun.

biologist is to furnish information about the environment of the fish or other marine organisms that are being studied. It is of course recognized that the charts of salinity and temperature are based on surface observations alone, and that these do not everywhere represent conditions in the depths. Nevertheless, in some regions, such as the southern North Sea, they are a fair representation of the whole water column at all times. In the northern North Sea, excluding the Norwegian coastal region, they represent conditions down to about 30 m. in summer and to the bottom in winter, broadly speaking. I have the impression, too, that in the northern half of the North Sea (again excluding the Norwegian Rinne) the distributions of salinity at the surface and in the depths resemble one another very closely. In any case there are sufficient data available on subsurface conditions to permit an estimate to be made of the general relation between surface and subsurface conditions and the degree to which one field is likely to be representative of the other. For instance, we find that in the deeper water of the northern part of the Southern Bight, surface and bottom temperatures may be expected never to differ from one another by as much as 3°C., and to be within $\frac{1}{4}$°C. of each other on about 8 occasions out of 10; the probability is a little below this in summer and well above it in autumn. On the other hand, for the deep water to the north of the Dogger Bank, the difference between surface and bottom temperatures must always be expected to be large in summer, of the order of 7° or 8°C., and (I speak here without having worked out the figures) the order of magnitude of the difference in winter is probably much the same as in the Southern Bight. Mr. Smed (6) has given us a paper on this subject at an earlier meeting and Dr. Dietrich (7) has published a paper describing the various regions of the North Sea and adjacent waters, as identified by the relation between surface and subsurface conditions. He shows that in the southern part of the North Sea, in rather more than the eastern half of the English Channel, and in most of the Irish Sea, the water column can be regarded as both homothermal and homohaline at all times. Excluding an area off the Norwegian coast, almost all the rest of the waters closely surrounding the British Islands are classifiable as homohaline at all seasons, and homothermal during the winter half of the year. This description is, undoubtedly, to be taken in the broad sense; local conditions may introduce local modifications into the general picture. For example, Dr. Goedecke (8) has recently shown that the water entering Helgoland Bight from the Elbe is distinguishable in a fairly limited region as an upper layer of different temperatures and salinities from those of the water beneath, and located in a region broadly classifiable as one of vertical homogeneity.

It may be felt that the synoptic charts have only a limited value to the biologist as representing the environment of the organisms he is studying, on the grounds that they do not precisely represent the conditions prevailing at the moment the biological observations were made. I think, however, that the principle that the best conception of the environment is given by a hydrographic observation made simultaneously with the biological one sometimes needs qualification. It suggests the idea of a stationary environment, and the high degree of accuracy with which the hydrographic

observations are usually made may give a false impression of the relation between the organism and the surrounding medium. It appears to me that the environment in such regions as the North Sea, for instance, is better regarded as one which is continually changing, and very rarely static. The tidal streams keep the water in a state of almost continuous turbulent motion, and the mixing produced by them and by the wind tends to fuse the different kinds of water together, so that the various water masses are not as a rule clearly and definitely identifiable. Even when recognizable discontinuities exist, no water mass is absolutely homogeneous in all respects. Moreover, the processes which promote changes in the hydrographic conditions are themselves constantly changing, from moment to moment, and from place to place. The effect on any organism, whether sessile or mobile, therefore, is that it lives in an environment which can be regarded as constant only very approximately. A temperature measured to one hundredth of a degree or a salinity determined to one hundredth of a part per mille, for example, can have very little significance, to this degree of precision, in representing the environmental conditions, for similar observations made a few moments later or at a short distance away will give different results. Accordingly, I would contend that a broader, coarser view of the environment such as can be obtained from synoptic charts may sometimes enable a more realistic appreciation of the environment to be made than can be furnished by discrete observations, even though made with great accuracy and simultaneously with the biological observations.

I should like now to invite attention to some of the hydrographic features shown by the charts.

Firstly, may I begin with a word of caution? The details which the charts can show depend not only upon the scale of the chart but also upon the distribution of the observations at hand. For instance, in the southern part of the North Sea, in about latitude 53°N., between the Wash and the Friesian Islands, it is not as a rule possible, through absence of data, to get a perfectly clear picture of the salinity field. In one instance, February 1952, we were able to distinguish two very narrow bands of water, one with salinity over $34{\cdot}6^0/_{00}$ and the other with salinity below $34{\cdot}4^0/_{00}$. These two bands lay extremely close to one another. The former is the extension of the salt water occupying the central region of the Southern Bight, and the latter is the extension of fresher water seawards from the Norfolk coast. They are apparent in the following month, though not so clearly distinguishable, but to what extent they may be considered as regular features of the salinity (and temperature) fields the data do not permit us to decide. This part of the North Sea, which is the meeting place of at least three different waters, is, I think extremely interesting to the hydrographer from the point of view of mixing processes, and one in which more might be profitably learnt about these processes.

Again, in the English Channel the charts show the salinity as decreasing fairly steadily from west to east. The charts of average conditions for the period 1903 to 1927 (9), however, show a more or less steady decrease as far as about 1°W. longitude; thereafter, salinity rises for some distance, falling again before Dover Straits are reached. This feature is apparent in all

but the last three months of the year. Its non-appearance in any of the present series of charts may be ascribed to absence of data from the region near 1°W., where the rise begins, but there is also another possible explanation.

It may be noticed that these charts seldom show a development of the low salinity area reaching southwards from the neighbourhood of the Scilly Islands, and the concomitant band of salter water stretching northwards between these islands and Land's End. In the charts of mean conditions (9), these features are noticeable throughout the year; they are particularly prominent in August and September, and least so from December to February. The present series of charts shows them in August and September 1950, and perhaps in January and February 1951. In August 1951 they are only very weakly developed and are absent in September. They appear again in February and March 1952. The data are not very widely distributed in this region, it is true, but I think that unless some considerable alteration of the salinity field has occurred in this locality, they are sufficient to indicate the presence of these features. If this is accepted, then the prominence of these features in the winters of 1951 and 1952 and their weak development in summer 1951 does in fact suggest a fairly considerable change, or abnormality, in the surface water circulation in this region. It may be the case that during this period what we would regard as the normal winter type of circulation prevailed in summer, and the summer type in winter. In the normal winter circulation (10) the water entering the English Channel appears to follow a predominantly easterly direction, in contrast with the north-easterly direction obtaining in summer. This may have had its repercussions in the eastern part of the Channel, producing the effects referred to above, namely, the absence of a rise in salinity in the eastern Channel.

In reviewing the Notes which accompany the charts, I gained the impression that temperature was recorded as being higher than usual rather more frequently than might be expected. Examination shows that as regards the general region covered by the charts, temperature is described, broadly speaking, as more or less above normal in 17 months out of the 24, and as more or less below normal in 7 months. These latter, incidentally, fall in the period December 1950 to July 1951. To estimate the anomaly for the region as a whole is a little difficult, however, but if the anomalies stated in the Notes for the English Channel are averaged, we arrive at a value of −0·6°C. This, of course, suggests that our standards of comparison are too low. In fact, comparison was made with charts based on mean values for the period 1903 to 1927 (9). The means of the temperature observations for the period 1928 to 1951 have been computed for the same regions as before, and the average difference between the values for the two periods is found to be −0·57°C. It is therefore interesting that the warmer conditions, the existence of which has already been noted by several investigators, are continuing, and also that, broadly speaking, they prevail over at least most of the region covered by the charts. Mr. Smed (11) has pointed to the predominance of positive temperature anomalies in the northern North Sea since about 1932; Dr. Goedecke (12) records an anomaly of −0·6°C. in the mean water temperature at "Elbe I" lightship, comparing the periods

1923–32 and 1933–50, and states that the mean temperature in Helgoland Bight generally has risen during the last 10 years or so. He also notices that conditions suggest a climate that has become more maritime than formerly at the offshore lightships and more continental at the inshore; the transition from one type to the other has sharpened, in other words.

From a study of the Ocean Weather Ship data, Dr. RODEWALD (13) has shown that in certain parts of the North Atlantic, temperatures from November 1950 to December 1951 have in general been above normal, by about 0·5°C. on the average. The positive anomaly is particularly high at Station D (ca. 44°N., 41°W.). At certain stations, however, between 50 and 55°N., the anomaly is negative, notably at Station C (ca. 53 N., 35 W.). From a study of the barometric conditions, Dr. RODEWALD concludes that these alterations in the North Atlantic temperature field may be attributed to "an intensified cyclonic movement of the subpolar watermasses", together with a shifting, rather than an intensification, of the Gulf Stream.

Perhaps one of the most striking features of the present series of charts is the remarkably regular appearance of relatively low salinities and temperatures in the western part of the Atlantic region, between 50° and 60°N., in about 20°W. This is very clearly shown by a comparison of the present salinity charts with the mean charts for an earlier period. West of Ireland, the 35·2‰ isohaline normally lies between 25° and 30°W., but in the present charts it appears never to be far from the meridian of 20°W., and is occasionally seen well eastward of it. It would seem that Dr. RODEWALD's suggestion of a changed water circulation, at any rate at the surface, is clearly supported by the present series of charts.

Unfortunately, I know of no subsurface observations from which to tell how deep into the water this state of affairs extends. It has persisted in the surface for so long a time that it seems reasonable to expect that it affects the deeper water also. It is interesting to speculate on the possible consequences of this. It is clear that the shift of less salt and cooler water towards the east has considerably narrowed the channel, if it may be termed such, by which relatively warm, salt water moves towards the north-east. If this water has maintained its usual levels, it follows that either a smaller volume of it has moved north-eastward or it has moved faster. On the other hand, it may have extended its levels to a greater depth, or spread more widely beneath the less salt and cooler waters that have moved across from the west. In this connexion, it is noteworthy that north of 60 N., salinities do not seem to have been unusually low, but rather the contrary, especially in the region south of Iceland and west of the Faeroes. Accordingly, the inference is that the north-easterly movement of warm salt water past the British Islands has either been faster or has reached to deeper levels than usual — the latter perhaps being the more likely.

This view appears to be supported by findings described in a paper (14) written since the present paper was read at Copenhagen. In the water beyond the 1000 m. isobath south-west of Ireland, the temperature at 200 m. has been considerably higher since 1929 than it was in the preceding quarter century. In the early 'thirties, the anomaly was about $+\frac{1}{2}$ C.; for later years, the only available observation, one made in 1950, shows an

anomaly of about $-\frac{3}{4}$°C. Despite the meagre data, it is clear that the rise in temperature is on so large a scale — it applies to all seasons of the year — that it is hardly attributable to local processes of conduction and convection, by which heat is carried down from the surface, especially in view of the comparatively great depth of the level effected; the deepening of the current, involving horizontal transfer of heat, seems much the more likely explanation.

Summary

For each month since June 1950, charts of surface temperature and salinity, and of currents and winds, in the North Sea and adjacent waters have been published, together with a descriptive note, about four months after collection of the data. The present paper concerns the charts covering the two years up to May 1952. The charts are restricted to the sea surface because not enough data are available for charts of subsurface levels. The extent to which surface observations represent subsurface conditions is considered. It is believed that as recording devices are developed for hydrographic observations the quantity of data will increase rapidly. The preparation of synoptic charts requires, not only sufficient data, but an organization adequate to handle them.

One of the main purposes of the charts is to employ the collection of hydrographical data in such a way that the fishery biologist can obtain early information of changes occurring in the marine environment. The kind of information which can be derived from the charts is remarked upon. It relates, for example, to matters such as the inference of water movements from salinity changes (the need for caution in this connexion is emphasized); the possibility of forecasting; the occurrence of a general rise of about $\frac{1}{2}$°C. in temperature in the English Channel (in keeping with that observed in the North Sea); and the apparent connexion between the persistently low salinity in the western part of the Atlantic region (15°–20° W.) and changes in the oceanic circulation.

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Some Recent Work on Dissolved Oxygen in Natural Waters

By

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The concentration of dissolved oxygen is one of the main factors to be considered when assessing the condition of a polluted water. Recently a considerable part of the work of the Water Pollution Research Laboratory has been concerned with improving methods for determining dissolved oxygen and studying the factors affecting its concentration in freshwater streams and estuaries. Some aspects of this work are summarized below.

Determination of Dissolved Oxygen

The method most widely used for determining dissolved oxygen in water is that of WINKLER (1888) or one of its modifications, in which iodine is liberated in an amount equivalent to that of the dissolved oxygen in the sample; this iodine is then titrated with a standard solution of thiosulphate. Use of starch as an indicator in this titration may lead to error, particularly in small samples or in very dilute solutions, because an appreciable concentration of iodine must be present before any blue colour can be detected visually. Of three electrical methods tried for determining the concentration of iodine more accurately, that depending upon an amperometric titration was found to be most satisfactory (KNOWLES and LOWDEN, 1953). The current flowing between a bright platinum electrode and a saturated calomel electrode, both immersed in the solution containing excess sodium thiosulphate, is measured with a micro-ammeter as standard potassium iodate solution is added; the end-point is reached when the current increases (Figure 1). Alternatively the point at which a steady current is attained when all free iodine has reacted with thiosulphate may be measured.

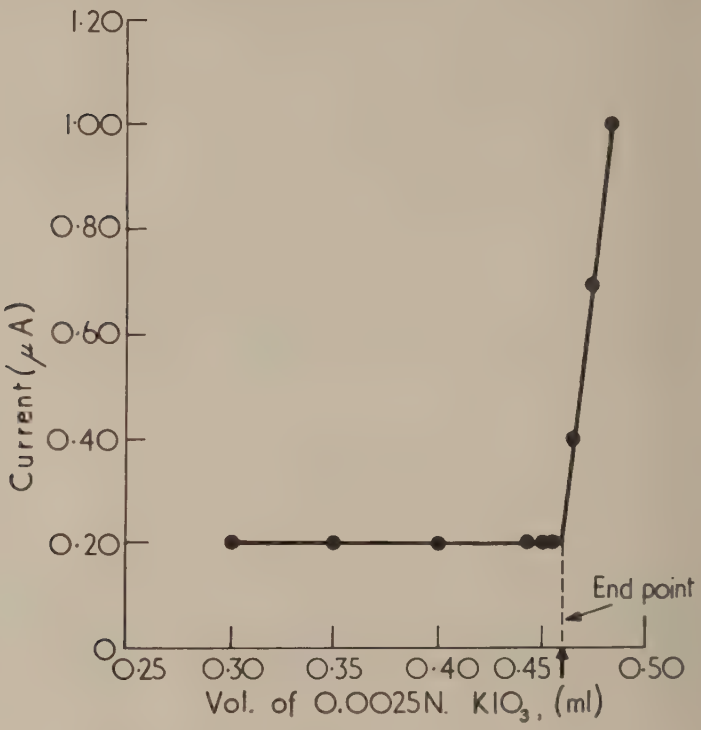

Figure 1. Amperometric titration of iodine and thiosulphate.

Table 1

Comparison of values for the oxygen content of distilled water and of sea water determined by a gasometric method and by a titrimetric method

Distilled water Concn. of oxygen, p.p.m.			Sea water, salinity 31·35‰ Concn. of oxygen, p.p.m.		
(a) Gasometric method	(b) WINKLER method	Difference between values (a–b)	(a) Gasometric method	(b) Azide modification of WINKLER method	Difference between values (a–b)
10·30	10·30	0	7·83	7·84	−0·01
10·14	10·11	+0·03	7·34	7·37	−0·03
9·49	9·47	+0·02	6·89	6·86	+0·03
9·29	9·27	+0·02	8·13	8·13	0
9·71	9·72	−0·01	7·88	7·86	+0·02
Mean of differences		+0·012	Mean of differences		+0·002

To determine whether the WINKLER procedure was of sufficient accuracy to use in critical work, the method has recently been compared with a gasometric reference procedure (WHEATLAND and SMITH, 1955) in which the dissolved gases in a sample of water are pumped out under vacuum and collected in a constant-volume gas-analysis unit; the volume and composition of the extracted gases are then determined. It was estimated that the maximum errors from all sources to be expected in this method were −0·037 or −0·04 p.p.m. oxygen. Five samples of distilled water were examined by this method and by the WINKLER method, the amperometric method being used for detecting the end-point; a similar comparison was made on five samples of sea water with a salinity of $31{\cdot}35^{0}/_{00}$ the azide modification of the WINKLER method being used to overcome interference by nitrite (ALSTERBERG, 1925). The results (Table 1) show that the difference between the two methods was less than the expected experimental error.

Automatic Recording of Dissolved Oxygen

In studying the concentration of dissolved oxygen in natural waters, the diurnal fluctuations may be important and analysis of samples taken during daylight only is of limited value. An apparatus which will record the concentration of dissolved oxygen over a period of 24 hours or more has been developed in the Laboratory (BRIGGS, KNOWLES, and SCRAGG, 1954); this can be used only when a source of electric power is available. A sample of water is taken automatically every 10 minutes; the reagents required for determining oxygen by the WINKLER method are added through valves operated in turn by a stepping switch, but instead of titrating the iodine with thiosulphate the optical density of the liberated iodine is measured by a photo-electric cell and the result is recorded on a chart.

Solubility of Oxygen in Water

The values for solubility of oxygen in pure and saline water usually quoted in books of reference are based on the results of gasometric determinations by FOX (1909). In laboratory experiments on the rate of re-aeration of de-oxygenated water it was found that the ADENEY and BECKER law (1919) was not satisfied when FOX's figures were used to calculate the saturation value. As no satisfactory explanation of this could be found it was decided to determine anew the solubility of oxygen in pure and saline waters in the temperature range 2° to 40°C. (TRUESDALE, DOWNING, and LOWDEN, 1955).

To do this a sample of water was first partially de-oxygenated by a stream of nitrogen or was supersatured by a stream of oxygen. A steady flow of air was then maintained over the surface of the water and the water was stirred until equilibrium was reached. The content of dissolved oxygen was determined by the WINKLER method using the amperometric end-point. It was found on each occasion that the same value was obtained whether

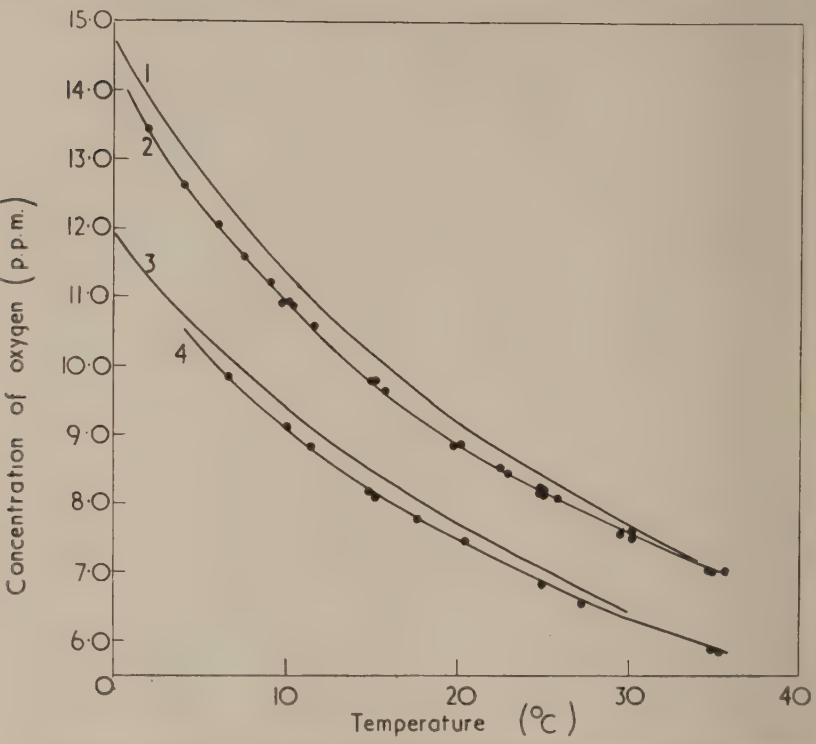

Figure 2. Solubility of oxygen in pure water and sea water (salinity $30^0/_{00}$) exposed to an atmosphere of wet CO_2-free air at a pressure of 760 mm., as calculated from measurements made by FOX and as determined by the WINKLER technique.
Curve 1. Pure water – calculated.
Curve 2. Pure water – determined.
Curve 3. Sea water – calculated.
Curve 4. Sea water – determined.

equilibrium was approached from above or below. The values found were lower than the accepted values by about 0·3 per cent. at 35°C. and by 3–4 per cent. at the lower temperatures (Figure 2). The figure obtained for solubility in distilled water at 25°C. has been confirmed independently by ALLEN (1955) working in Tasmania.

On the assumption, generally accepted, that the solubility of oxygen changes linearly with salinity, the solubility in saline water can be expressed by the equation

$$C_s = 14{\cdot}161 - 0943T + 007714T^2 - 0{\cdot}0000646T^3 - S\,(0{\cdot}0841 - 0{\cdot}00256T + 0{\cdot}0000374T^2)$$

where C_s is the solubility, in p.p.m., of oxygen in water of salinity $S^0{}_{00}$

and T is the temperature in °C. If great accuracy is not required, the following simpler equation (GAMESON and ROBERTSON, 1955) may be used

$$C_s = \frac{475 - 2{\cdot}65S}{33{\cdot}5 + T}$$

In the temperature range 0°–30°C. this equation is believed to give the true solubility to within 0·1 p.p.m.

When the new values are applied to the determination of the rate of solution of oxygen in water, satisfactory agreement with the ADENEY and BECKER law is obtained.

Acknowledgement

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Synopsis

Recent work at the Water Pollution Research Laboratory, concerned with the concentration of dissolved oxygen in natural waters, has included use of an amperometric method for determining the end-point in the WINKLER method for determining dissolved oxygen, a check of the accuracy of the WINKLER method against a gasometric procedure, development of an apparatus for the automatic recording of dissolved oxygen, and a correction of the accepted values for the solubility of oxygen in water.

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Monthly Charts of Surface Salinity in Indonesian and Adjacent Waters

By

Klaus Wyrtki

In 1949 the Laboratorium Penjelidikan Laut (Marine Research Laboratory), Djakarta, started an extensive programme of observing surface salinity and temperature in Indonesian and adjacent waters. One of the objects was to obtain data for the investigation of salinity fluctuations in these waters. The yearly monsoon cycle produces changes in the movements of the water masses, which are likely to be reflected in the distribution of surface salinity. These water movements may be expected to have considerable bearing on the movements of fish and of their eggs and larvae, as we have learned from investigations in the waters of north-west Europe. The programme is undertaken in accordance with the recommendations of the Indo-Pacific Fisheries Council as part of the research on fishery biology planned for these waters. The data will not only provide the basic material for oceanographical investigations, but will also make a considerable contribution to practical fishery research. The development and intensification of fisheries in this area is a very urgent task.

Quasi-synoptic charts of the distribution of various properties of the upper water layer (salinity, temperature, and nutrients) now have an important rôle in fishery research and may later come to play a similar part in practical fishery. Since these charts are, at present, published several months after collection of the observations, they do not yet possess an immediate use in practical fisheries. It is to be hoped that as sampling methods are developed, the charts will be made available much earlier. To illustrate the value which these charts already have in fishery research, reference may be made to the investigations made in the waters of north-west Europe. There comprehensive fisheries statistics are available, as well as a large number of hydrographical observations. Investigations, especially those

Chart showing the localities named in the text.

of J. N. CARRUTHERS and collaborators (1951), have shown that a close connexion exists between hydrographical conditions and brood-strength or catch of fish. Future studies will no doubt demonstrate further correlations between the winds, the currents, and the distribution of the fish and their brood. The first results of these investigations are already of use to practical fishermen in these waters.

In Indonesian waters the fishery is in an early stage of development and a comparison between the yield and the hydrographical conditions is not yet possible. Before this can be done both hydrographical data and fisheries statistics must be collected. But above all, the principles governing the relations between the monsoons and the water movements must be studied.

The collection of sea water samples, for later titration ashore in the Laboratory, is undertaken by ships of several different lines which operate regularly in these waters. Whenever a sample is collected the water temperature is taken also and the usual meteorological observations are made. The Laboratorium Penjelidikan Laut receives about 1200 salinity samples a month from the area between 20°N. and 15°S., and 90° and 145°E. The distribution of the samples over this area is naturally very uneven. There are many samples from the main shipping routes, but from rarely visited localities the observations are few.

The data so obtained are being recorded in a card index, which also contains all earlier observations of surface salinity. Some of these earlier data were used by P. VEEN (1952) for monthly charts of surface salinity from March 1949 to February 1950. Charts for the period March 1950 to February 1953 have been prepared by J. HARDENBERG and R. SOERIAATMADJA and are now being printed. P. VEEN (1953) also calculated monthly surface salinity means for these waters, for which he used all data up to December 1951 then available. But the number of observations for the three years 1949 to 1951 is so much in excess of the number for former years that his charts in fact show approximately the average conditions for these three years, because the general monthly means were computed from all observations, irrespective of year, and not from the monthly means of the separate years.

The samples are titrated in the Laboratory by a micro-method, described by G. ARNOLD (1951), which only needs 1 ml. of water. This is of great convenience with so large a number of observations. The method is said to have an accuracy of $0{\cdot}1\,^0/_{00}$ in salinity, with titrations made against standard sea water. This precision is just sufficient for the purpose of the present investigations. The local rainfall and evaporation tend to produce rapid changes in salinity, and more precise analyses have shown that differences in salinity up to $0{\cdot}2\,^0/_{00}$ often occur in the upper 5 to 10 metres of water, and that considerable horizontal differences are the rule. The diurnal variation of the surface salinity sometimes amounts to $0{\cdot}1\,^0/_{00}$. In general, the salinity fluctuations in these waters are so large that the most precise methods of analysis hardly seem to be necessary. Moreover, the data are treated statistically for the most part, so that by taking mean values minor irregularities are smoothed out. As a rule the period covered is a month, in the course of which considerable variation may take place.

From January 1955 onwards the Laboratorium Penjelidikan Laut has regularly published monthly charts of surface salinity for the sea area between 20°N. and 15°S., and 90° and 143°E. These charts cover the waters of the Indonesian Archipelago, the China Sea, and the waters round the Philippines, that is to say, the whole area between the Indian and Pacific Oceans on the one hand and between Asia and Australia on the other. (The region is shown in the chart, p. 269, which also indicates the location of the various geographical features referred to in the text.)

The charts used are on a rectangular projection, with a scale of 1:15 million. The salinity observations for the month are averaged for each 1° square and entered on a working sheet, on which the isohalines are subsequently drawn. The number of observations differs considerably from month to month; sometimes there are from 10 to 40 samples for each 1° square in the Java Sea, the south China Sea, and Malacca Strait, whilst other squares are represented only by single observations. In the charts, a dot indicates only one observation in a 1° square, and a cross, several observations. To avoid confusing the picture, the isohalines are drawn for intervals of $1^0/_{00}$, although the salinity ranges only from about $34{\cdot}5^0/_{00}$ in the oceanic water in the Indian and Pacific Oceans to $30^0/_{00}$ or less in Malacca Strait and off the mouths of the large rivers of Kalimantan (Borneo). However, the distribution of surface salinity in these waters is so complicated that isohalines drawn at closer intervals would only overcrowd the charts. But in regions of oceanic influence in which the horizontal salinity gradient is less steep, the 33·5 and $34{\cdot}5^0/_{00}$ isohalines are sometimes drawn also, if necessary. As an example, the chart for May 1954 is reproduced on a reduced scale in Figure 1. The number of observations used for this chart is 1360. They are distributed over 339 squares in the following way:—

Number of observations	1	2	3	4	5	6–9	10–19	>20
Number of 1° squares	121	75	39	27	13	33	19	12

The chart shows the salinity distribution at the change-over of the monsoon seasons. At this time the north monsoon, which reaches its maximum in the months of December to February, has stopped, but the south-east monsoon has not yet developed fully. The rainy season in the Indonesian Archipelago has ended and the salinity has reached its minimum. The $34^0/_{00}$ isohaline has retreated to the Indian and Pacific Oceans. There is no connexion between the waters of salinity over $34^0/_{00}$ in the two oceans. From the Java Sea low-salinity water ($32^0/_{00}$) advances eastward and almost reaches the coast of Sulawesi (Celebes) and Flores. The China Sea is filled with relatively salt water of over $33^0/_{00}$, which has come from the north. Similarly water of over $34^0/_{00}$ has come from the Pacific Ocean into the Sulawesi Sea and, through the Philippines, the Sulu Sea. The monsoon in this particular year must have been stronger or more continuous than usual, because the $32^0/_{00}$ isohaline in the Flores Sea lies more to the east, and the $33^0/_{00}$ isohaline in the south China Sea more to the south than they are shown by P. Ch. Veen's (1953) mean charts for May.

Since 1950 the International Council for the Exploration of the Sea has published charts for north-west European waters showing the surface

LABORATORIUM
PENJELIDIKAN LAUT
PASAR IKAN
DJAKARTA - KOTA
INDONESIA
BIRMA
THAILAND
CAMBODJA
LUZON
MINDANAO
KALIMANTAN
SUMATRA
SULAWESI
IRIAN
JAVA
TIMOR
AUSTRALIA
MAY 1954
SURFACE SALINITY ‰
single observation in 1°square
several observations in 1°square

distributions of both salinity and temperature, as well as the winds and currents in the area. These charts encouraged the Laboratorium Penjelidikan Laut to publish the salinity charts for south-east Asian waters. Although charts of temperature distribution would have been possible, they were not undertaken, because the temperature fluctuations in this area are relatively small, in contrast to those in north-west European waters. It is not possible to provide a good representation of the wind conditions, because the available observations are too scanty and the conditions are nearly everywhere complicated by the daily sequence of land and sea-winds. Moreover, the computation of the geostrophic wind from the pressure distribution, as can be done for the North Sea, is not possible in low latitudes. This defect can only be remedied by further investigation of the relation between the fields of pressure and wind in these low latitudes, and it may then become possible to reach satisfactory estimates of the strength of the monsoons, which so markedly affect the water movements in these waters.

Variation in Salinity Distribution

The surface salinity of the oceanic water masses in the Pacific and Indian Oceans is subject to only small annual variation. Between these oceans there is a region in which the salinity is in general lower but has a higher annual variation. This is caused by the considerable discharge from rivers, by changes in the difference between precipitation and evaporation, and by the changes in the current systems due to the monsoons. The variation of these factors varies from year to year and causes considerable deviation from the mean annual variation of surface salinity, although the annual variation is the predominant character.

In the open parts of the Pacific and Indian Oceans the salinity always exceeds $34^0/_{00}$, while in the western Java Sea and in the southern part of the China Sea, where the effect of the discharge from rivers becomes important, it varies between 30 and $32^0/_{00}$. Large areas with salinities below $30^0/_{00}$ are to be found only in the Straits of Malacca, off the river mouths on the coasts of Sumatra and Kalimantan, and in the inner part of the Gulf of Thailand. The frequency of occurrence of various salinities in the area under consideration is shown in Figure 2. Three water types are clearly distinguishable, oceanic water with salinities over $34^0/_{00}$, mixed water with salinities between 32 and $34^0/_{00}$, and coastal water with salinities below $32^0/_{00}$. The mixed water occupies about 60% of the area, and the two other water masses together only occupy 20% each. If water of less than $30^0/_{00}$ salinity is called "river" water, we see that "river" water amounts to only 3% of the area. Accordingly the fluctuations of surface salinity may be demonstrated by the movements of the $34^0/_{00}$ and $32^0/_{00}$ isohalines. The annual cycle, as shown by the observations over the period 1950–1954, is as follows:—

Oceanic water with salinities over $34^0/_{00}$ advances each year from the Indian and Pacific Oceans into the Indonesian Archipelago (Figure 3). The Indian Ocean water retreats to its furthest limits in April to June, about

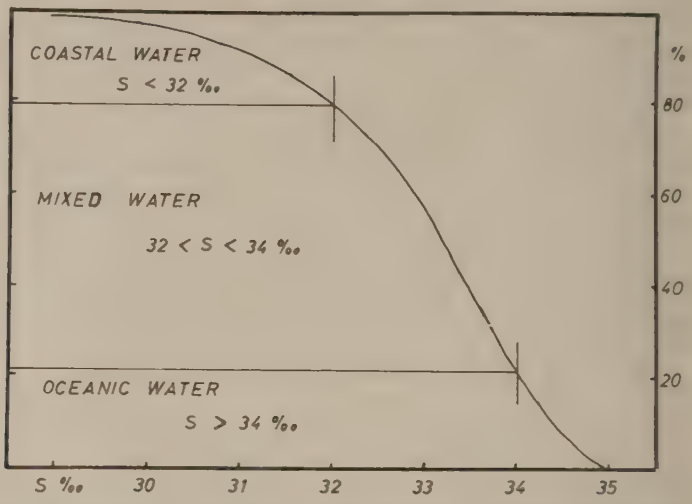

Figure 2. Frequency of occurrence of various salinities in south-east Asian waters.

two months after the end of the rainy season. The $34^0/_{00}$ isohaline then lies outside the southern arc of the Sunda Islands in about 9°–12° S. When the dry south-east monsoon starts, the isohaline moves from this position into the Arafura, Banda, and Flores Seas. At the same time Pacific Ocean water, coming from the north-east between the Philippines and Irian (New Guinea), penetrates the Sulawesi Sea. The two water masses first meet in July or August, between Sulawesi and Irian, and then again in the Straits of Makassar one month later. In August under the steady influence of the south-east monsoon, the $34^0/_{00}$ isohaline moves further westward into the Java Sea and reaches the limit of its advance in September at the end of the dry season. In some years water of over $34^0/_{00}$ does not quite reach the shores of Irian and the large bays on the eastern side of Sulawesi, and may also stop short of the south coast of Java and the west coast of Sumatra. In the northern part of the region, in the waters round the Philippines and in the northern entrance to the China Sea, the conditions are not so regular. Between March and September the Sulu Sea is often only partly filled with water over $34^0/_{00}$, some of which enters between the Philippines, and some comes from the Sulawesi Sea. The maximum advance of the $34^0/_{00}$ isohaline from the north into the China Sea coincides with the maximum of the north-east monsoon, between December and February, but it varies a good deal in extent.

While the yearly course of salinity in the eastern part of the archipelago is governed by oceanic water, conditions in the western part depend on coastal water with salinities lower than $32^0/_{00}$ (Figure 4). From the chart showing the retreat of the $32^0/_{00}$ isohaline we learn that there are three areas in which the coastal waters originate, the Gulf of Thailand, the

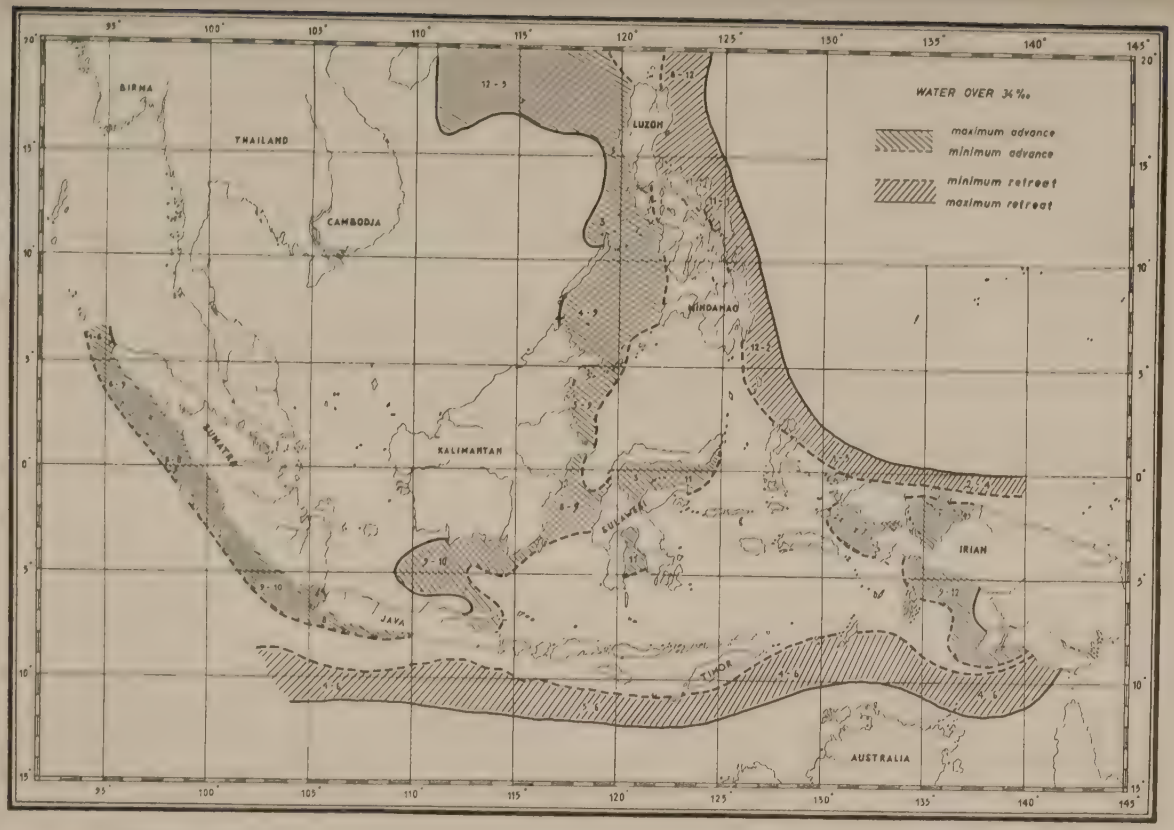

Figure 3. Movement of the 34‰ isohaline during the years 1950–54. (The numerals indicate the months in which the positions are reached.)

Figure 4. Movement of the $32^0/_{00}$ isohaline during the years 1950–54. (The numerals indicate the months in which the positions are reached.)

Straits of Malacca, and the river mouths on the west and south coasts of Kalimantan. The date at which the maximum retreat occurs in these localities varies according to the time of the dry season, but in general it falls between April and October. After this time, with the beginning of the rainy season low-salinity water penetrates the southern China Sea from both sides. When the north monsoon begins in October, this water is carried by the general circulation from the southern China Sea into the Java Sea and further eastward. At the end of the rainy season (February–April) it covers the whole of the Java Sea and the Straits of Makassar, and extends as far as the region south of Sulawesi. Water of less than $32^0/_{00}$ also occurs every year on the southern coasts of Java and the western coasts of Sumatra.

In order to indicate the areas in which the coastal water originates the maximum advance of the $30^0/_{00}$ isohaline is shown separately in Figure 5. It is astonishing how far this water can extend seawards. The largest area

Figure 5. Maximum advance of the $30^0/_{00}$ isohaline in the years 1950–54. (The numerals indicate the months in which the positions are reached.)

covered by it seems to be in the Gulf of Thailand, but lack of observations makes it impossible to verify this. The second largest area is off the mouths of the rivers of Sumatra, the outflow from which permanently maintains an area in the Straits of Malacca at below $32^0/_{00}$ (see Figure 4). When the rivers are in spate, an area half as large as Sumatra can be covered with water under $30^0/_{00}$. The third, but none the less important, area is situated off the coast of southern Kalimantan, where the $30^0/_{00}$ isohaline often spreads up to 100 miles seawards. The maximum advance of the $30^0/_{00}$ isohaline varies a little, according to the rainy season in the different river basins. It occurs about one month after the end of the rainy season.

The primary cause of the fluctuations in the surface salinity appears to be the water movements produced by the monsoons. During the north-east monsoon oceanic water of high salinity is brought from the Pacific into the China Sea, and during the south-east monsoon water from the Indian Ocean is carried into the eastern part of the archipelago. However, the annual fluctuations in rainfall and evaporation, which are very large in this locality, govern the form of the annual cycle in surface salinity.

The difference between precipitation and evaporation (P − E) reaches an average of 20 cm. month in this region, but in particular months values up to 35 cm. month may occur (ALBRECHT, 1951). If the quantity of salt in a layer of thickness H and salinity S is to remain constant, it follows that $\triangle S \cdot H = - \triangle H \cdot S$, where $\triangle S$ is the change in salinity corresponding to a value $\triangle H = (P - E)$. With a thickness H of 30 m., an average salinity

of 33‰, and an excess of rainfall over evaporation of 20 cm./month the fall in salinity in the course of a month is given by:—

$$\triangle S = -\frac{\triangle H}{H} S = \frac{-20}{3000} \cdot 33 = -0{\cdot}22‰$$

In the course of a rainy season a change of 1‰ in the salinity of the upper 30 m. of water is possible as the result of the difference (P − E) alone. As a rule changes in surface salinity seldom penetrate deeper than 30–50 m., as observations in this area have shown. An additional factor affecting the surface salinity distribution is the discharge from rivers, which is greatest during the rainy season, as is shown by Figure 5.

In general it can be said that there are several factors which influence salinity and it is their combined effects which determine the salinity distribution shown in the monthly charts. But further research is necessary before the effects of each factor can be assessed quantitatively and before the surface salinity distribution in south-east Asian waters can be completely understood. The monthly surface salinity charts now published by the Laboratorium Penjelidikan Laut should be a valuable contribution to this study.

Summary

As part of a programme of fishery research planned by the Indo-Pacific Fisheries Council, the Laboratorium Penjelidikan Laut, Djakarta, has undertaken, since 1949, the regular collection of surface salinity and temperature observations in Indonesian waters. The water samples, together with temperatures and meteorological data, are obtained by commercial vessels. The salinity titrations are carried out ashore by a micro-method requiring only 1 ml. of water. Since salinity in these waters varies widely, the accuracy of 0·1‰ claimed for the method suffices for the purpose intended.

From January 1955 onwards the Laboratory has published monthly charts of surface salinity for the area 20°N.–15°S., 90°–143°E. For the most part isohalines are required only for intervals of 1‰. Temperature has not been similarly charted, because it fluctuates relatively little in this area.

Four types of water may be distinguished — oceanic water of over 34‰, coming alternately from the Pacific and Indian Oceans under the influence of the north-east and south-east monsoons; coastal and "river" waters with salinities below 32 and 30‰, respectively, which derive from the large excess of precipitation over evaporation in the region; and mixed water, of salinity between 34 and 32‰. The last type is the most common and occupies about 60% of the area.

The changing pattern of salinity distribution through the year reflects, on the one hand, the changes in direction of water movement brought about by the monsoon sequence, and on the other, the seasonal variation in the difference between precipitation and evaporation. Other factors are at work, however, and further study is required before all the effects can be assessed quantitatively and the nature of the distributions fully understood.

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Considerations on Productivity[1]

By

E. J. Ferguson Wood

Productivity may be defined as light energy transformed by photosynthesis into energy-containing plant materials, and thus rendered available for further biological use. As far as we know, this is the only means whereby energy is added to the oceans, for all other processes are energy-consuming. Autotrophic bacteria utilize the free energy of chemical reactions such as the oxidation of sulphur to synthesize organic material, but in this case, the energy is largely derived from the earth; that is, there is no gross gain in energy. From the fact that non-biological energy gives rise to biological synthesis, all autotrophic nutrition could be regarded as productivity, but the earlier definition seems to me more satisfactory.

Productivity is only one aspect of the cycle of life in the sea, and philosophically speaking should not be studied as an isolated entity. However, it is a parameter that carries some meaning and can give us some data which is of immediate use. Measurement would seem to be of first importance in assessing the total organic matter that can be produced in the oceans of the world, and in allowing comparison of ocean regions as possible sources of food.

Because the oceans are so vast, and man's excursions upon and into them are so puny, quantitative studies of the sea require a number of assumptions, and it seems apropos to consider some of these. In the case of inorganic materials, we know that we can apply the laws pertaining to solutions to our chemical studies of the sea, and that, provided sampling is adequate, we can obtain fairly satisfactory answers to our scientific questioning. With biological transformations, however, we are not in such a happy position. In the first instance, we are dealing with a suspension of particles of matter on which substances and even micro-organisms may be adsorbed, the adsorption facilitating certain reactions at the expense of others. Rubentschik *et al.* (1936), ZoBell (1943), and Wood (1953) have shown the importance of adsorption in the distribution and nutrition of bacteria in the sea, and there can be no doubt that adsorption plays a similar part with

[1]) The considerations discussed in this paper emerged from work carried out as part of the programme of the Division of Fisheries of the Commonwealth Scientific and Industrial Research Organization, Australia.

the smaller flagellates. There is evidence that the particulate matter is not randomly distributed, thus greatly increasing the sampling error. As about 90 per cent. of all photosynthetic micro-organisms in the sea will pass through a No. 1 Whatman filter paper, the importance of adsorption as a factor in sampling error will be manifest.

Next comes the tendency of plankton organisms in the sea to occur in swarms or blooms. Such blooms are conditioned by both chemical and biological factors, and while we can estimate the chemical factors with some plausibility, we know little of the biological ones, which lead to blooms of certain species.

As a further difficulty in quantitative studies of the seas comes the high cost of getting samples far from the shore, and there are vast tracts of the oceans that have never been visited by a scientific expedition. All this means that we have a serious obstacle to overcome at the start of our investigations, that of adequate and representative sampling.

Now let us consider the aims and methodology of productivity studies. Should we, and can we, assess productivity as a standing crop or dynamically? The methods now used are based on taking a presumably representative sample, and determining some parameter of it. They may be divided broadly into chemical and biological methods. The former include the estimate of phosphate, nitrate, chlorophyll, etc., the latter, respiration and photosynthesis by techniques developed by GRAN (1927), RILEY (1938), and others, and the ^{14}C technique of STEEMANN NIELSEN (1952). Of themselves, all these techniques estimate the standing crop, and tell us nothing of the dynamics. True, the biological methods rely on the behaviour of the organisms in the sample after capture, but the similarity between the behaviour of organisms in the sample and in the sea ceases as soon as the sample is separated from the sea. ZOBELL (1943) has shown how bacteria are affected when placed in glass vessels, but no one has studied the behaviour of photosynthetic organisms under similar conditions. Another point is that all, or nearly all, the organisms of the phytoplankton are capable of heterotrophic existence, and the apparent need for soil extract in the cultivation of plankton micro-organisms suggests that the hardiest of them are ergon-heterotrophic. The fact that phytoplankton organisms behave differently when removed from the sea is shown by the difficulty one has in growing them in culture. If one takes a plankton sample and places it in a culture medium, be it sea water or enriched sea water, the organisms dominant in the culture will normally be very different from those in the original sample. Nor will duplicate cultures of the same sample necessarily contain the same species as dominants. For this reason, the biochemical assessments of productivity do not appear to have absolute significance, but can only be used to compare samples. A criticism of the ^{14}C technique is that it takes only an arbitrary account of carboxylation, i. e., the assimilation of carbon dioxide without photosynthesis. In heterotrophic nutrition, a certain proportion of carbohydrates is first converted to carbon dioxide by the WOOD-WERKMAN phenomenon, i. e., carboxylation is a normal assimilation route. Before STEEMANN NIELSEN'S technique can be accepted, it must be shown that ^{14}C is quantitatively assimilated by photosynthesis under the conditions of the experiment, and

that the Wood-Werkman assimilation and re-utilization of respired carbon dioxide are reasonably constant.

Estimations of productivity by chlorophyll determinations do not take into consideration heterotrophy, the possible accessory action of accessory pigments (Blinks, 1954) or the effect of depth on these factors.

These criticisms of the methods are not intended to discourage their use, but to guard against complacency, and to suggest lines of investigation which may improve the usefulness of such methods.

The estimations of standing crop by such methods as those discussed above may be likened to the quantitative sampling of grass in a field. We can learn how much grass there is at a given time, but the estimation tells us nothing of the number of animals in the field or of the intensity of the grazing. There is a further analogy — if a field is grazed, the herbage grows differently from that of an ungrazed field, and we cannot assess the annual yield of a field by studying the unchecked growth of the grass. Braarud has made some studies of the rate of reproduction of diatoms, etc., and Clarke and Gellis (1935), Harvey (1934), and Raymont and Gross (1942) have studied the grazing rate of *Calanus*, but there are not sufficient data for us to estimate the rate of reproduction of phytoplankton as a whole or to have any definite ideas on the rate of grazing of zooplankton. ZoBell believes that the paucity of bacteria in the sea may be due to heavy grazing. Our knowledge of such matters is far too fragmentary to allow us to interpret estimates of standing crop in terms of dynamics, yet dynamic processes are the ones with which we are concerned either as oceanographers or as fishery scientists.

Fleming (1939) has tried to produce an equation for relating standing crop to total annual productivity. His figures are based on the diatom plankton alone, and ignore the micro-plankton, which has more recently been shown to be of the greatest significance. However, he has stated:— "The necessity for more adequate knowledge concerning the division rates of plankton diatoms and the effect of environmental factors on these (for diatoms read phytoplankton organisms) is pointed out. It is also shown that more information is necessary concerning the feeding mechanism of zooplankton grazers and that the determination of their filtration capacity under various environmental conditions are essential." Since this was written, very little has been done in either of these fields, largely due to lack of suitable methods. I cannot find in the literature a record of the culture of *Rhizosolenia* or of the truly oceanic *Ceratia*, and I myself have found difficulty in getting reproducible cultures of *Chaetoceras*. Cultures of some of the smaller flagellates have been made by Cole (1936), Bruce, Knight, and Parke (1940), and others, but the methods are not reliable enough to let us determine with certainty the generation times of all the more important phytoplankters or to provide adequate cultures for studying the feeding rates of zooplankton grazers.

It would seem that planktologists need to spend a good deal of effort on culture experiments, the results of which, when correlated with actual observations on phytoplankton reproduction and grazing in the ocean, would give a quantitative meaning to such equations as that suggested by

FLEMING. Then, and only then, can we get an estimation of plankton population dynamics which will be more than an inspired guess.

It seems probable that the cycle of animal life in the sea is largely a closed one in which the photosynthetic organisms are not concerned, and which may be likened to a child's hoop. Productivity, in the sense in which I have defined it, is the hand that bowls the hoop and supplies the energy lost by friction. The existing animal population of the sea depends on past productivity while its maintenance (and that is the important thing for us) depends on present productivity.

Summary

The study of productivity in the sea, i. e., the light energy converted to biological material, presents many difficulties to the student. Sampling is difficult and expensive because of the vast extent of the oceans and the non-random distribution of phytoplankton. Methods used have serious errors, especially as the majority of phytoplankters are facultative heterotrophs, even though they contain chlorophyll. Another limiting factor is our lack of knowledge of reproduction rates of the plants concerned, and of the grazing rates and selectivity of feeding of the zooplankton elements.

It is suggested that more attention should be paid by planktologists to studies on generation times of phytoplankters and on grazing rates, and on the influence of heterotrophy.

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Real and Apparent Changes in Size of Marine Animal Populations during the Australian Fisheries Investigations

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I. Introduction

Biological work in Australia on properly identified populations of fish and other marine animals has been continuous only since 1937. The record of catches and fishing effort as a whole is no older, although there are a few series going one or two decades further back which can be identified with certain populations.

Several populations of fish and other aquatic animals have now been identified in Australian waters, and for some of these it has been possible to compare apparent abundance (apparent population size) from year to year over a period of several years. It is of interest to assemble and briefly review the results of this work, to which several investigators have contributed, to see what regularity or irregularity there is in them and what significance this may have for future work on such populations.

No estimates of actual size of any of these populations, most of which are large and widely distributed, are available; there are only density or frequency indices for different years, which indicate, at best, comparative time changes in the state of each population. Three kinds of indices, the first two being density indices and the third a kind of frequency index, have been used; (i) commercial catches, adjusted wherever possible for amounts of fishing effort; (ii) catches from research vessels, similarly adjusted; and (iii) numbers of days on which part of the population was encountered (in this case as shoals at the surface of the sea) by research vessels, expressed as percentages of numbers of days devoted to the search. The limitations of (i) are well known and probably are not very important for the present

purpose, which is to detect gross time changes or their absence. The limitations of (ii) and (iii), which are obviously much greater, were recognized by the authors mentioned in Sections III and IV of this paper, which deal with those indices. Reference is made in Section V to some anecdotal evidence of changes in apparent population size.

Records of catch and fishing effort have been obtained from scientific papers cited in the text and from published and unpublished records of State Government fisheries authorities. All species not otherwise identified are fish (Pisces).

II. Information from Commercial Catches

(a) Sea Mullet (*Mugil cephalus* LINNAEUS): Eastern Australian Population. The species is catadromous. The population was recognized by KESTEVEN (1942) and its history as revealed by catch records was discussed by THOMSON (1953). These records are available for the period commencing with the year 1938/39 and are summarized in Figure 1 (A). THOMSON (1953) stressed the lack of full data on fishing effort with which these catches might have been

Figure 1. Recorded annual catches of (A) eastern Australian sea mullet population, (B) western Australian sea mullet population, (C) Bass Strait barracouta population, and (D) eastern Tasmanian barracouta population. For further explanation, see text.

compared; he could neither confirm nor deny the conclusion of KESTEVEN (1942) that the population was, or at one stage had been, declining because of overfishing; and he drew attention to the rather stable level of the catch. The greatest recorded annual catch was only about half as large again as the smallest.

(b) Sea Mullet: Western Australian Population. The probability that the Western Australian individuals of this species constitute a single population, separate from that of eastern Australia, was discussed by KESTEVEN (1942) and THOMSON (1951). THOMSON (1950) examined the catch and effort records, which were available only from 1941 onwards for the population as a whole, and was unable to confirm the belief of KESTEVEN (1942) that the population

had been declining as a result of overfishing. Figure 1 (B) shows that the level of catch from year to year has been fairly stable; if the years 1942 to 1945, in which fishing was deliberately restricted, are disregarded, the greatest recorded annual catch was only about twice the smallest.

(c) Barracouta (*Thyrsites atun* (EUPHRASEN)): Bass Strait Population. The species is pelagic. BLACKBURN and GARTNER (1954) recognized the population and showed that complete catch records were available only for 1941/42, 1942/43, and the years from 1947/48 onwards. Good records of fishing effort relating to the population as a whole were not available before 1948/49, but observations on the demand for the fish permitted some assessment of the amounts of effort expended at certain other periods. From this it was concluded that the rise in the catch which began after 1942/43 and reached a peak at 10·4 million lb. in 1948/49 (Figure 1 (C)) reflected an increase in effort, not necessarily in amount of fish available, and that the fall between 1948/49 and 1949/50 reflected a decline in effort. However, the further fall from 6·6 to 3·6 million lb. in the following year, and the failure of the catch to improve in the years after that, cannot be so explained and indicate a fall in the supply of fish.

This fall appears to be a natural fluctuation in availability of the population to the fishermen, who search for it only in a few localities very close to the coasts of Bass Strait, and not a change in the abundance of fish in the population as a whole — i. e., a change in the distribution or behaviour, not the size, of the population — for reasons given by BLACKBURN and GARTNER (1954). The main reason is that the scarcity is more marked at some places and at some stages of each annual cycle of the population than at others; for instance, it is little evident at the beginning of the annual cycle.

(d) Barracouta: Eastern Tasmanian Population. The population was recognized by BLACKBURN and GARTNER (1954). Catch records, reported by them and brought up to date in Figure 1 (D) of the present paper, were available for the same years as for the Bass Strait population. Data on effort were available for the period commencing with 1947/48 and showed that the rises and falls in the annual catch reflected changes in supply of fish. The only one of these changes which was important was that of 1953/54, when the catch was about one-sixth of the recorded maximum in 1948/49 and about one-fifth of the catch of the preceding year 1952 53. The cause is not known; however, several fishermen reported in 1953 54 that the fish were abundant but would not bite, so again a fluctuation in availability may be suspected.

(e) Commercial Scallop (*Notovola meridionalis* (TATE)) and **Doughboy Scallop** (*Mimachlamys asperrimus* (LAMARCK)): D'Entrecasteaux Channel, Tasmania, Populations. These species are lamellibranch molluscs. FAIRBRIDGE (1953) gave reasons for assigning all scallops of each species in this area to the same population, and discussed the series of catch and effort records which date from 1929. Further pertinent data were given by OLSEN (1955). The catch and effort data presented by FAIRBRIDGE (1953), slightly revised and brought up to date[1]), are summarized in Figure 2; the fishing

[1]) By courtesy of Mr. A. M. OLSEN of the Division of Fisheries, C.S.I.R.O., who is preparing a further paper on this subject.

season was usually four months in each year, and when it was less the catch per unit effort figure (but not that of total catch) was increased to put it on a 4-month basis. There was little fishing before 1929, and the steady rise in catch and catch per unit effort in the first few years after that possibly reflect growth and technical improvement of the fishery rather than an increase in size of the exploited populations. However, there are three well-marked maxima (1937–1941, 1947–1949, and 1953–1954) and two distinct minima (1942–1945 and 1951–1952) in these curves which denote real changes in the supply of *N. meridionalis*; the other species has been comparatively rare since about 1940.

These changes, furthermore, are real changes in population size, due to varying numerical strengths of year-classes, as shown by FAIRBRIDGE (1953) and OLSEN (1955): the second maximum represents good year-classes of 1939, 1940, and 1941, the third maximum the good year-class of 1948, and the second minimum reflects the weakness of year-classes of the period 1942–1947.

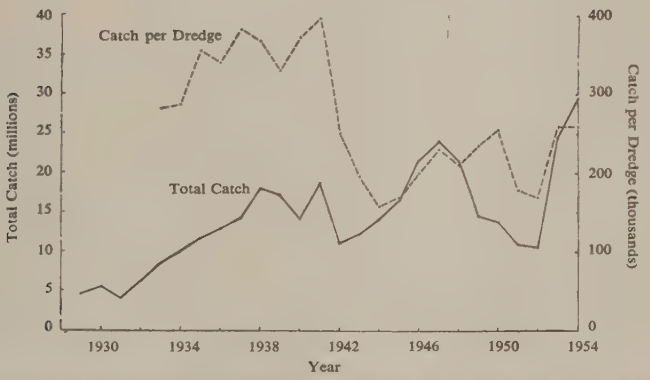

Figure 2. Recorded annual catches and catches per fishing dredge of the scallop population of D'Entrecasteaux Channel, Tasmania. For further explanation, see text.

Here, then, instability is more marked than in the populations discussed above. The fluctuations in the population of *N. meridionalis* are frequent, although only of moderate amplitude (the greatest catch per unit effort little more than twice the least), and reflect real changes in population size; there has also been an important change in the population of *M. asperrimus*.

(f) Southern Bream (*Mylio butcheri* MUNRO): Gippsland Lakes, Victoria, Population. The species is estuarine. BUTCHER (1945) showed that the catch of bream in this area had gradually fallen since 1919, much more than the amount of fishing effort. His records date from 1913. He gave information which makes it seem probable that all the bream of these connected lakes (which are open to the sea) belong to the same population and have little to do with those of other areas, and that the decline in the catch reflects a real fall in population size. BUTCHER attributed this to despoliation of the environment by man and also to overfishing; his data suggest that the former

was the primary cause. The greatest catch per unit effort was about 15 times the smallest.

(g) Australian Salmon (*Arripis trutta* (BLOCH and SCHNEIDER)): Eastern Australian Population. The species is coastal-pelagic. FAIRBRIDGE (1951a) distinguished two subspecies of *A. trutta*, which are here assumed to be not further divisible into populations, in Australian waters. The eastern Australian population occurs in Queensland (rarely), New South Wales, Victoria, and Tasmania, and the western Australian population in Western Australia, South Australia, Victoria, and Tasmania. As the catch records of Victoria and Tasmania cannot be partitioned between the two populations, and as trustworthy records are not available for the South Australian fishery, the only useful catch data are those for New South Wales and Western Australia, which relate entirely to the eastern and western populations respectively. These two states provide between them over three-quarters of the total Australian catch of *A. trutta*.

Figure 3. Recorded annual catches of Australian "salmon" in (A) New South Wales and (B) Western Australia.

The New South Wales catches for successive years from 1938/39 to 1953/54 are shown in Figure 3 (A). No study has been made of the effort expended in this fishery, but demand is believed to have been fairly constant over the period. There is also a good deal of stability about the catch, although that of the best year is about 11 times that of the worst. Changes in availability (behaviour) of shoals of Australian salmon have been observed from time to time in particular localities on this coast (BLACKBURN and TUBB, 1950) but not enough is known about the population in general to justify any attempt to interpret Figure 3 (A).

(h) Australian Salmon: Western Australian Population. The only trustworthy catch records are those for Western Australia, as explained above. They are summarized in Figure 3 (B). The fishery is recent and landings before 1948 reflect its growth and technical improvement rather than any change in the fish population. The moderate fall in the annual

catch from 1948 to 1952 is believed to reflect changes in the availability of the salmon shoals, which no longer came so freely within range of the fishermen's beach seine nets (FRASER, 1953). Since 1952 the catch has improved once more. The greatest annual catch was about twice the smallest.

(i) Tiger Flathead (*Neoplatycephalus macrodon* (OGILBY)): New South Wales Population. The species is demersal. FAIRBRIDGE (1951 b) could find no reasons for recognizing more than one distinct population of this species in New South Wales waters. FAIRBRIDGE (1952) and HOUSTON (1955) discussed trends in the catch and catch per unit effort data of the population, which are available, with several important breaks in the series, from 1918/19 onwards. The generally downward trend from 1929 to the present was held by both authors to indicate decline in the size of the population, resulting from overfishing. The greatest catch per unit effort was about 10 times the smallest. Neither author found any positive evidence of marked natural fluctuations in the strength of year-classes, although HOUSTON (1955) assumed the existence of such fluctuations to explain minor variations in the level of catch and catch per unit effort for which he could not otherwise account.

(j) Whitebait (*Lovettia seali* (JOHNSTON)): Northern Tasmanian Population. This population of anadromous salmonoids was identified by BLACKBURN (1950), who assembled the records of catch and effort (available from about the beginning of the fishery in 1943–1944) and showed that the size of the population decreased from 1946 to 1948, presumably from overfishing. The upward trend in catch and catch per unit effort until 1946 reflects growth and technical improvement of the fishery. The greatest catch per unit effort was about four times the smallest.

(k) School Shark (*Galeorhinus australis* (MACLEAY)): South-East Australian Population. OLSEN (1954), who identified this elasmobranch population, referred briefly to statistical and anecdotal evidence of a decline in its size, presumably due to overfishing. He quoted a catch maximum of 4,000,000 lb. and a subsequent fall to 2,500,000 lb.

III. Information from Catches by Research Vessels

(a) Southern Bluefin Tuna (*Thunnus maccoyi* CASTELNAU): Eastern Australian Population. The individuals of this little-exploited pelagic species which occur off the New South Wales coast and east coasts of Victoria and Tasmania appear to be the younger members of a single population (SERVENTY, 1941). From 1938 to 1941 this juvenile part of the population was sampled by troll-line fishing from research and semi-commercial vessels in coastal waters, with results as reported by SERVENTY (1947). Allowing for differences in the amount of fishing effort, fish were slightly more plentiful in 1939 than in 1940 and twice as plentiful in 1939 as in 1938 or 1941. These results could indicate fluctuations in the size of the sexually immature segment of the population but it is at least as likely that they indicate changes in its availability in surface coastal waters. The second possibility may be appreciated from the fact that the two best-represented

age-groups did not usually occur in the expected proportions in the catch (the senior group was usually commoner than the junior, whereas the reverse would be expected) and from differences in northward penetration along the coast from year to year (SERVENTY, 1947).

IV. Information from Observations of Surface Shoals

(a) Pilchard (*Sardinops neopilchardus* (STEINDACHNER)): Southern New South Wales Population. BLACKBURN and TUBB (1950) presented data on the frequency with which surface shoals of this unexploited pelagic species were seen in the optimum seasons in various years, regard being had to the numbers of occasions on which they were looked for, in New South Wales waters south and north of Port Jackson (Sydney). BLACKBURN (1951) concluded, partly from certain features in these data (discussed below) and partly from vertebral counts, that there were two populations in New South Wales waters, one in each of the above-mentioned (i. e., southern and northern) areas.

For the southern population there are two series of annual frequency indices of the type described above, (a) for 1-year fish for the four years 1938–1941 and (b) for older, mostly 2-year, fish for the four years 1939–1942 (BLACKBURN and TUBB, 1950). Each series is heterogeneous, as shown by χ^2 tests; and both are alike and significantly correlated, the frequency index being lowest for the first year and highest for the third year in each series. The combined data thus bear the interpretation of a succession of year-classes of very varying numerical strengths (1937 low, 1938 medium, 1939 high, 1940 medium) through the population, causing marked changes in actual population size. The data are not sufficient for any precise comparison of the numerical strengths of these year-classes.

(b) Pilchard: Northern New South Wales Population. Similar data for this population over the five years 1938–1942 show no signs of any fluctuations such as those apparent in the southern population at the same period; the differences in the frequency indices from year to year are not significant, according to a χ^2 test (BLACKBURN and TUBB, 1950).

Similar observations on frequency of occurrence of shoals are available for other species in certain areas (BLACKBURN and TUBB, 1950), but not for properly identified populations.

V. Anecdotal Information

There is a great mass of anecdotal and semi-scientific information about time changes in apparent abundance of other Australian aquatic animals of interest to man, mainly indicating increasing scarcity of these forms. Much of this information is suspect and very little is useful, as pointed out by BLACKBURN (1953). Two instances which are better documented than most may be mentioned here, although they refer to species and not necessarily to homogeneous populations.

One instance concerns the semi-demersal Tasmanian kingfish (*Rexea*

solandri (CUVIER and VALENCIENNES)). According to JOHNSTON (1883, 1891) this species was extremely common in shallow coastal waters of south-eastern Tasmania about 1880, and possibly at other times in the nineteenth century, but by the time of MCCULLOCH (1915) it was rare in those waters and has remained so ever since, although it is not uncommon in deeper waters (at and beyond the edge of the continental shelf) in the same area. In other parts of Australia it is taken at similar depths, e. g., in 150 to 250 fathoms off the New South Wales coast. It is probably a deep-demersal species which invades the shallow waters when conditions there are temporarily suitable, and not a coastal species.

The second case is that of a decapod crustacean, the tiger prawn of eastern Australia (*Penaeus esculentus* HASWELL). DAKIN (1938) summarized the fragmentary information from scientific writers about the abundance of this species in the waters of the central coast of New South Wales, which showed that it had been more abundant between 1880 and 1898 than it was in 1910 or at any time between 1928 and 1938; the situation has not appreciably changed since DAKIN wrote. The cause of the change is not known.

VI. Discussion

Annual catch or catch per unit effort data are available for 12 populations. Disregarding such figures when they refer to the early period of exploitation of a population and thus are unlikely to have had much biological significance, and taking catch per unit effort data in preference to catch figures where possible, one may divide these populations into two groups.

There are seven populations in which the maximum was not more than twice the minimum—the two mullet populations, the Bass Strait barracouta population, the commercial scallop population, the western Australian salmon population, the shark population, and the tuna population. Even if all the variations signified fluctuations in population size, one could scarcely say that these populations fluctuated markedly, especially as only one of them, the commercial scallop, exhibits more than one complete cycle of decline and recovery. But actually it is not certain that any of these populations, except the scallop population, have fluctuated naturally even as much as the catch data suggest, and for at least three of them other well-founded explanations are available for the changes that have been observed, namely, fluctuation in availability for the barracouta and Australian salmon, and depletion for the shark.

There are five populations in which the maximum was not less than four or more than fifteen times the minimum—the whitebait population (4:1), the eastern Tasmanian barracouta population (6:1), the flathead population (10:1), the eastern Australian salmon population (11:1), and the bream population (15:1). If all the variations signified natural fluctuations in population size, one could say that these populations, or most of them, fluctuated markedly, although none of them exhibits more than one complete cycle of decline and recovery. But actually it is not certain that any of these populations have fluctuated naturally to any marked degree, and for at least

three of them other well-founded explanations are available for the changes that have been observed, namely, depletion for the whitebait and flathead, and despoliation of environment for the bream. The two remaining populations, those of barracouta and Australian salmon, may have fluctuated in size; but it is at least as likely that the changes in catch represent fluctuations in availability, and no account has been taken of possible changes in fishing effort for the Australian salmon.

Similar assessments can be made of three other populations for which catch data are not available—the doughboy scallop population and the two pilchard populations. Two of these have fluctuated markedly and the other, the northern New South Wales pilchard population, appeared to be stable during the period in which it was studied.

Thus three populations, two of lamellibranchs and one of fish (southern New South Wales pilchard), have been found to fluctuate markedly either in amplitude or frequency of fluctuations, although not in both. Two fish populations, eastern Tasmanian barracouta and eastern Australian salmon, may have fluctuated similarly, although it is not certain that either did. Three more fish populations, namely whitebait, flathead, and bream, have declined markedly in abundance in a way that indicates human interference rather than natural change. The remaining seven of the fifteen populations discussed in Sections II, III, and IV — two populations of mullet, one of barracouta (Bass Strait), one of Australian salmon (Western Australia), one of tuna, one of pilchard (northern New South Wales), and one of shark — have fluctuated only slightly, if at all, in size.

Attention is drawn to the nine fish populations which appear to have been free from excessive human interference. Only one clearly fluctuated in size to a marked extent while six, equally clearly, did not. Whatever the other two populations did, it is obvious that marked natural fluctuation in population size has been the exception rather than the rule in the Australian fish populations investigated over the last two decades. It is also clear that no instance has yet been found of fluctuation on the grand scale reported by HJORT (1914), BJERKAN (1930), SETTE (1931), and others for various fish populations in high latitudes in the North Atlantic, where large annual changes in catch are sometimes very frequent and can be shown to reflect real fluctuations in population size, although periods of comparative stability over one or two decades are not unknown. This is surprising in view of the small number of year-classes present at any one time in most Australian fish populations investigated (see KESTEVEN 1942, BUTCHER 1945, BLACKBURN 1949 and 1950, THOMSON 1951, FAIRBRIDGE 1951 b and 1953, and HOUSTON 1955 for length of life in populations), as the fewer the year-classes the more the unusually great or small contribution of any one of them would be reflected in population size.

It is suggested that this infrequency of well marked fluctuations in size of investigated populations of Australian fish, compared with those of higher latitudes in the northern hemisphere, is an example of the phenomenon of "reduced variability in complex ecosystems" (SOLOMON, 1949, and references there; DICE, 1952, pp. 351–2). Warm waters such as those around Australia, which lie to the north of the subtropical covergence, support a more varied

fauna and flora than cold waters and the occurrence of organisms is not on the whole as markedly seasonal as it is in cold waters. It is probable that aquatic animals in warm waters, both young and old, have at any particular time a wider choice of prey and a greater variety of predators than similar species in cold waters, which would tend to stabilize the sizes of populations to a greater extent in warm seas than in cold.

This hypothesis cannot be rigorously tested at present because many more fish populations have been identified and investigated in high latitudes (all in the northern hemisphere) than in low. There is at least one clear case of a major fluctuation in a fish population in latitudes comparable with Australian, the recent decline of the pilchard (*Sardinops caerulea*) of the Pacific coast of North America, although heavy fishing probably contributed to this by reducing the number of year-classes in the population (Clark, 1952). Reference is sometimes made to striking fluctuations in a tropical clupeoid fish, *Sardinella longiceps*, the oil sardine of the west coast of India (Nair and Chidambaram, 1951; Nair, 1953). The annual variations in the catch of this species are indeed most marked and frequent, with as many as three major rises and falls in a decade and the greatest catch nearly 100 times the smallest; however, the literature shows that these changes have not been definitely identified as variations in population size, and that it is at least as likely that they represent fluctuations in the availability of shoals in the restricted areas in which the fishermen, whose boats are very primitive, can operate.

It is hoped that the increasing volume of work on tropical and subtropical fish populations in various parts of the world will, in time, sustain or reject the above-mentioned hypothesis.

Finally, it may be pointed out that fluctuations in availability of populations for fishing operations, i. e., in their distribution and behaviour within and between years, appear to affect the Australian fisheries at least as much as either overfishing or natural fluctuation in population size. Such changes occur in populations of barracouta, Australian salmon, and tuna (Sections II and III; see also Robins, 1952, as regards temporary extension of range of the striped tuna, *Katsuwonus pelamis* (Linnaeus)) and must, in fact, be comprehended before more precise statements can be made about possible changes in the real size of these pelagic populations. This point was more fully discussed by Sette (1950) who stated that "... the solution of problems of availability now constitutes the foremost task with respect to pelagic fisheries. It is a field where progress has been very limited." The problem is probably just as important to workers on estuarine fish, demersal sea fish, and crustaceans.

This paper was prepared as part of the research programme of the Division of Fisheries, C.S.I.R.O. (Australia).

VII. Summary

Series of annual density or frequency indices are available for fifteen properly identified animal populations of Australian marine waters — twelve populations of fish, two of lamellibranchs, and one of elasmobranchs. Three

populations, including the two lamellibranchs, exhibited marked natural fluctuations in population size; three had markedly declined, probably as a result of interference by man; seven had fluctuated little, if at all; and the remaining two instances were doubtful. No population has been found to fluctuate as much as some well known populations in high latitudes in the northern hemisphere.

It is suggested that this comparative infrequency of well marked fluctuations in size of investigated populations of Australia is an example of the phenomenon of reduced variability of population size in complex ecosystems, warmer seas being biologically more complex than cold.

It is also apparent that natural fluctuations in availability of populations for fishing operations affect the Australian fisheries at least as much as either overfishing or natural fluctuations in population size, particularly for pelagic populations.

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On the Selection of Hake and Whiting by the Mesh of Trawls

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Introduction

Recently the study of the release of small fish through the meshes of the trawl, particularly of the cod-end, has received considerable attention. This paper describes principally experiments carried out on hake and whiting on grounds to the south of Ireland, but also includes data for whiting in the southern North Sea. The ship concerned was the research vessel "Sir Lancelot" using mainly the standard Lowestoft trawl. The ship and gear, and also the method of attaching shrimp-netting covers to the cod-end, are described in detail by MARGETTS (1955). Also used in the Irish experiments was a trawl made to the specifications of a French whiting trawl; this differed from the Lowestoft trawl chiefly in having a longer funnel and cod-end, longer and more heavily floated head-line, and in having rather smaller meshes throughout. Vigneron-Dahl gear, with 30-fathom bridles, was used in all hauls to the south of Ireland, but not in those in the North Sea.

Previous mesh experiments on hake have been carried out by DAVIS (1934), using covered cod-ends, and by LETACONNOUX (1955), who compared the catches of 4 ships, each using a different mesh. DAVIS gave 50% release points of 21 cm. and 28 cm. for meshes of spool-size 80 mm. and 100 mm. respectively; these meshes correspond to meshes, as measured with a gauge, of approximately 64 mm. and 88 mm., giving ratios of 50% point: mesh-size of 3·15 and 3·2 respectively. LETACONNOUX gave 50% points of 27 cm., 35 cm., and 42 cm. for mesh-sizes (as measured on a gauge) of 60, 70, and 80 mm. respectively; these give ratios of 4·5, 5·0, and 5·25.

Table 1

Measurements of cod-end meshes during the 1953 and 1955 experiments

Mesh Gauge Size (mm.)	"A" 1955 Scissor	"C" 1955 Scissor	1953 Wedge
61	–	1	–
62	–	–	–
63	–	–	–
64	–	3	–
65	–	4	–
66	–	8	–
67	–	3	–
68	1	17	1
69	2	14	
70	3	12	
71	5	15	5
72	6	11	8
73	10	6	10
74	14	3	13
75	19	1	15
76	32	–	11
77	24	–	5
78	23	–	1
79	16	–	1
80	9	–	1
81	7	–	1
82	1	–	–
83	2	–	–
84	1	–	–
85	–	–	–
Total	175	100	78
Mean size	76·3	69·3	74·0

Covered-net experiment

Three different cod-ends were used in a series of hauls in August 1955 in 40 fathoms off the Smalls. The meshes were measured with a spring-operated scissor-type gauge of a type being developed at the Lowestoft laboratory. The jaws of the gauge are inserted into the lumen of the mesh, and are forced apart by a spring, which gives a separating force of about 7 lb. The subjective element occurring with the more usual wedge-type gauge, due to differences in the force used, is thus eliminated. The outward pressure is rather greater than that normally achieved with a wedge-gauge, and a comparison showed that it thus gave a mesh-size 1 mm. greater. In this paper therefore the mesh-sizes observed have been reduced by 1 mm. to bring them into accordance with an average wedge-gauge measurement, the actual observed mesh being, where applicable, given in parentheses. The meshes used were 70 mm. (69 mm.) and 77 mm. (76 mm.). The measurements of the meshes in these cod-ends, and of that used in the North Sea experiments are given in Table 1.

With the last of these cod-ends 16 hauls with the Lowestoft trawl were made with the upper part of the cod-end covered with shrimp netting. In

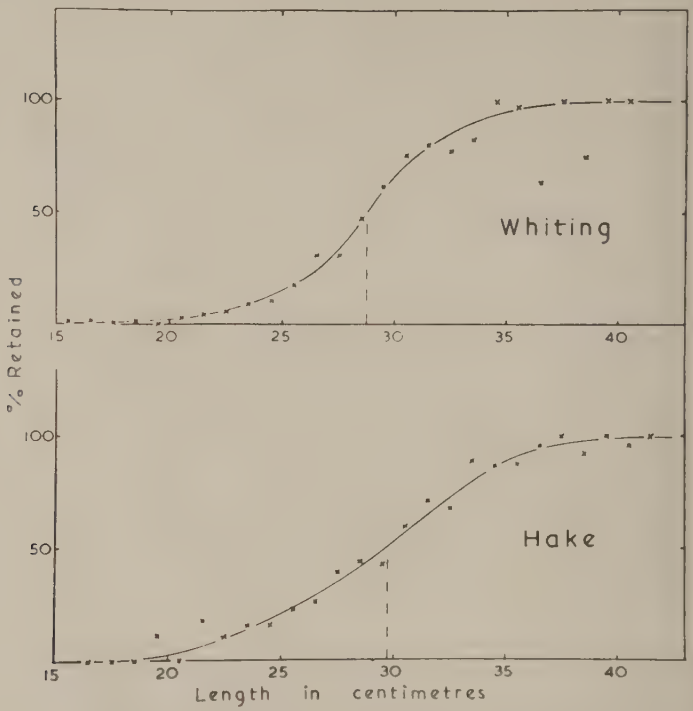

Figure 1. Selection of hake and whiting by a 77 mm. cod-end.

these hauls some 2,000 hake and 7,000 whiting were caught, the bulk of both species being between 20 cm. and 35 cm. long. In Figure 1 the percentages of each length-group retained in the cod-end have been plotted for hake (below) and whiting (above). Reading off from this graph gives a 50% point of 29·7 cm. for hake and 28·7 cm. for whiting.

With covers on the 70 mm. cod-end 13 hauls were made on the same grounds with the whiting trawl, and 3,500 hake and 10,000 whiting were caught. The selection curves for the two species are shown in Figure 2. The resulting 50% points are 26·1 cm. for hake and 27·1 cm. for whiting.

In a previous series of experiments 24 hauls had been made with the Lowestoft trawl, without Vigneron-Dahl gear, on grounds in the North Sea about 25 miles NE. from Smith's Knoll, with a 74 mm. cod-end (as measured with an ordinary wedge-gauge) and a shrimp-netting cover. The principal species caught was whiting, which numbered some 5,000 fish, and the corresponding selection curve is given in Figure 3. The 50% selection point read from the curve is 23·6 cm.

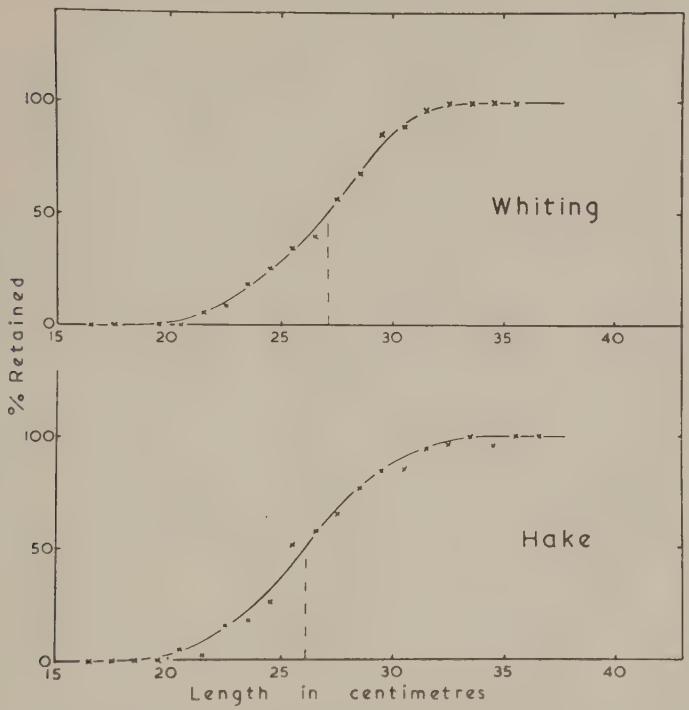

Figure 2. Selection of hake and whiting by a 70 mm. cod-end.

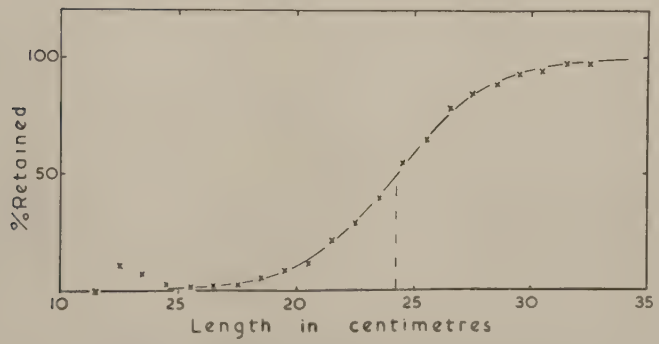

Figure 3. Selection of whiting by a 74 mm. cod-end.

Alternate hauls

A small number of hauls were made using the 77 mm. and 63 mm. cod-ends alternately, with the purpose of checking whether there was any serious discrepancy between this method and the covered cod-end method, rather than determining independently a definite 50% point. Throughout the series the 60 mm. cod-end was left uncovered, but for the first four hauls the 77 mm. cod-end was covered — the data from these hauls being incorporated with other covered-net data in the previous section — after which a further three hauls were made with it uncovered. Thus 7 hauls were made with the large-meshed net, and 6 hauls with the other, but by using the data from one haul with the small net twice (that haul during which the covers from the large-meshed net were removed), two series, one of 4 pairs of hauls and one of 3 pairs of hauls, were obtained. In Figure 4 the ratio of the catches in the cod-ends of the large and small meshes for the two series have been plotted against length for the whiting (above) and hake (below). In the figure data for lengths above 34 cm. have been combined to reduce fluctuation due to small numbers. The results from the two series are reasonably similar. For the uncovered hauls the apparent 50% points are approximately 29·8 cm. (whiting) and 30·6 cm. (hake). These are about 1 cm. greater than those obtained from the whole series of covered hauls, though Figure 7 below shows that the figures are no higher than those obtained from several individual covered cod-end hauls; that is, the admittedly few alternate hauls revealed no striking discrepancy. Figure 4 shows that for both hake and whiting (particularly the latter), the larger mesh (when uncovered) caught more large fish (above 31 cm.) than the small mesh. Though the difference is not statistically significant, it is in accordance with the results of other experiments, and may be reasonably explained by the better flow of water through the larger mesh. With the larger mesh covered with shrimp netting and hence presumably the flow of water impeded, the larger mesh caught fewer whiting, but more hake, than the small mesh. A further effect that might be expected to appear from Figure 4 is the possible masking of the meshes of the cod-end by the cover. If this takes place there should be more small fish in the cod-end. The figure shows that this may have occurred for whiting between the lengths 23–30 cm., though not for hake.

Escape through other parts of the trawl

For four hauls with the whiting trawl a cover was put over the baitings just in front of the cod-end. It was about 8 feet long and covered the width of the trawl. The catches, in numbers of hake and whiting in the cod-end and the two covers, are given in Table 2.

The length distributions of catches in the two covers were very similar. The results are in agreement with those of TODD (1911), using a beam trawl, in suggesting that though escape does take place through other parts of the trawl, escape through the cod-end meshes makes up the major part.

The effect of girth

In practically all mesh experiments the size of fish has been recorded in terms of the length — normally in terms of the centimetre below, this

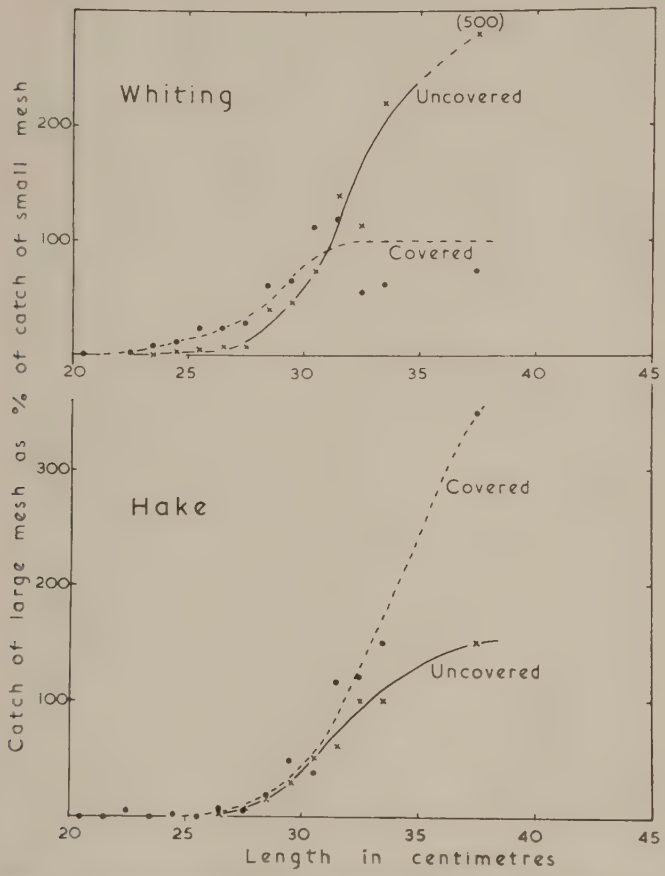

Figure 4. Ratio of catches of hake and whiting by alternate hauls with large- and small-meshed cod-ends, the series of hauls in which the large cod-end had a shrimp-netting cover being distinguished.

Table 2

Catches of hake and whiting in the cod-end of a trawl, and in covers over cod-end and baitings

	Whiting				Hake			
Haul. No.	24	25	26	27	24	25	26	27
Cod-end	484	320	100	61	88	50	30	47
Cod-end cover	404	810	588	228	71	86	48	42
Baitings cover	35	56	42	26	3	4	4	2
% in baitings cover	3·9	5·0	6·1	9·0	1·9	2·9	5·1	2·2

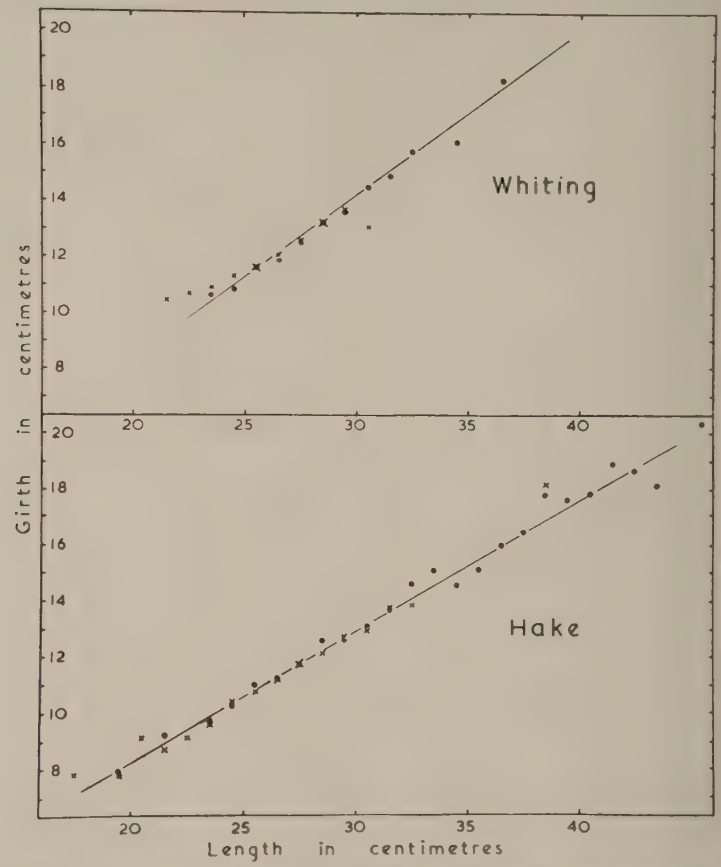

Figure 5. The length-girth relationship for hake and whiting, the fish caught in the cover (×) and in the cod-end (•) being distinguished.

measure being both quicker and less liable to subjective error. However, it has long been suspected that a fish's girth, rather than its length, is a major factor in determining whether or not it escapes through the meshes. MARGETTS (1954) showed that for haddock and whiting, the girth of a fish at the 50% selection point was about, but rather below, the internal diameter of the mesh. GRAHAM (1954) has suggested that the 50% release point for any fish can be estimated simply from a knowledge of its girth. To study this problem the girths of a large number of hake and whiting caught to the south of Ireland in 1955 were measured, the fish from the cod-end and the cover being kept separate. The girths were measured with a tape measure drawn firmly, but not tightly constricted, about the body

of the fish. The resulting data are shown in Figure 5, in which the mean girth at each centimetre length-group is plotted for hake (below) and whiting (above). At each length the mean girth of the fish in the cover is denoted by a cross, and of fish in the cod-end by a dot. For neither species does there appear to be the least difference between the girth-length relationship between the two sets of fish — in cod-end and cover. A straight line has been fitted by eye to a combined set of points for each species. The line for whiting fits almost exactly the points of MARGETTS's (1955) Table 2, for the natural girth of North Sea whiting. Reading off from the line gives the girths corresponding to the 50% release point for lengths as 12·8 cm. (hake) and 13·4 cm. (whiting) for the 77 mm. cod-end, and 11·1 cm. (hake) and 12·5 cm. (whiting) for the 70 mm. cod-end. For both cod-ends the girth at 50% point of whiting is greater than that of hake; i. e., a whiting will get through the mesh when a hake of the same girth will not. This may well correspond to the greater vigour and activity of whiting, as observed when the catch arrives on deck. This lack of difference between cod-end and cover length-girth relationship is contrary to that expected, which was that for fish of a given length within the selection band the latter would, on the average, be found still in the cod-end, and the slimmer would have escaped through into the cover. A possible explanation may be that for at least a majority of fish within the selection range, the critical factor on which their escape depends is whether they can take the first step and get their head into the lumen of a mesh, instead of lying broadside on to the netting. For this a short length, rather than a small girth would be an advantage. That many fish may not find themselves in such a position is suggested by the comparatively small proportion of fish slightly above the selection size which became meshed behind the gills in the manner of gill-nets, even though those hake which were meshed were fixed firmly. In fact this meshing of marketable hake can become a problem because they are often damaged in being removed from the net. Though the proportion observed to be thus damaged during the present experiment was low — only a few per cent. — this could amount to several boxes of fish during a commercial voyage. In contrast to the hake, the proportion of soles just above the selection point which become meshed is very high, probably not far from 90%, and in good fishing for soles nearly every mesh of the cod-end seems when hoisted inboard to have a sole's head pushing through. If this process is a fair description of the mechanics of selection, at least for round fish, then the duration of haul may noticeably affect the selection — the longer the fish is in the net, the better chance it will have of finding itself in a suitable position (with its head opposite the centre of a mesh) to escape.

To test this hypothesis first suggested to me by R. J. H. BEVERTON, the duration of haul was varied, from ½ hour up to 3 hours. For each haul the length distribution of cod-end and cover catches were plotted in the same figure, and a 50% point estimated from the point of intersection of the two curves. For most hauls the data were sufficient to define a 50% point in this manner to within one or two millimetres. A striking feature of this analysis was the fact that two consecutive hauls, apparently under identical conditions, often gave 50% points differing much more widely than could

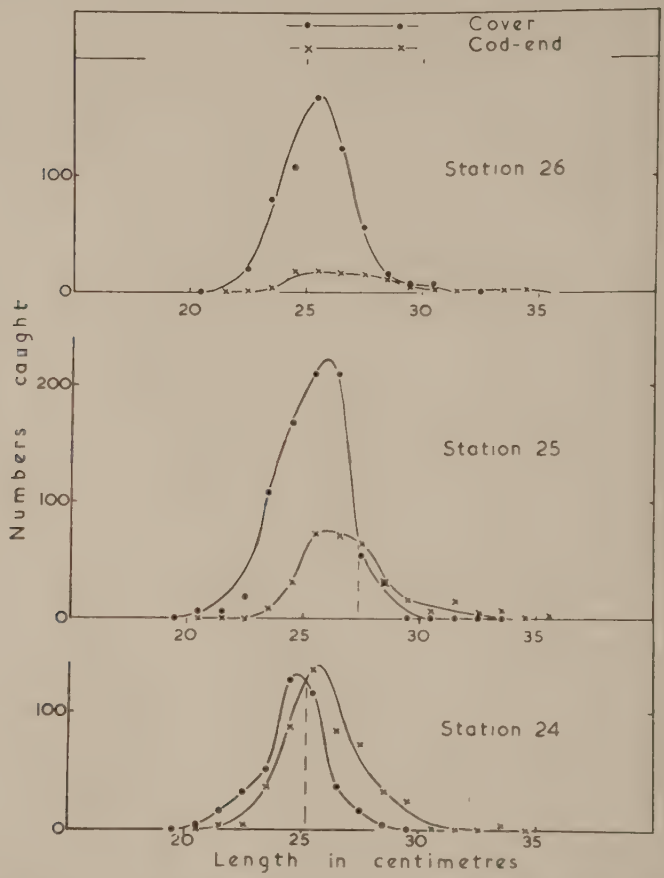

Figure 6. Catches of whiting in cod-end and cover in three successive hauls under apparently similar conditions. The change in selection is shown.

be accounted for by ordinary sampling errors. An extreme example of such variation is provided by the whiting catches of hauls 24–26 of the 1955 cruise. These are illustrated in Figure 6; in each haul the catches in the cover are very similar, but those in the cod-end are widely different. Allowance being made for possible random sampling errors (which should be small, in that the catches number over 600 in each haul) and possible differences in drawing the curves, wide limits for the 50% points for the 3 stations are 24·8–25·4 cm., 27·1–28·3 cm., and at least 29 cm. These were consecutive hauls with the same rig of gear, each of 2 hours, over the same ground. Catches of hake and whiting in these hauls are given in Table 2; other

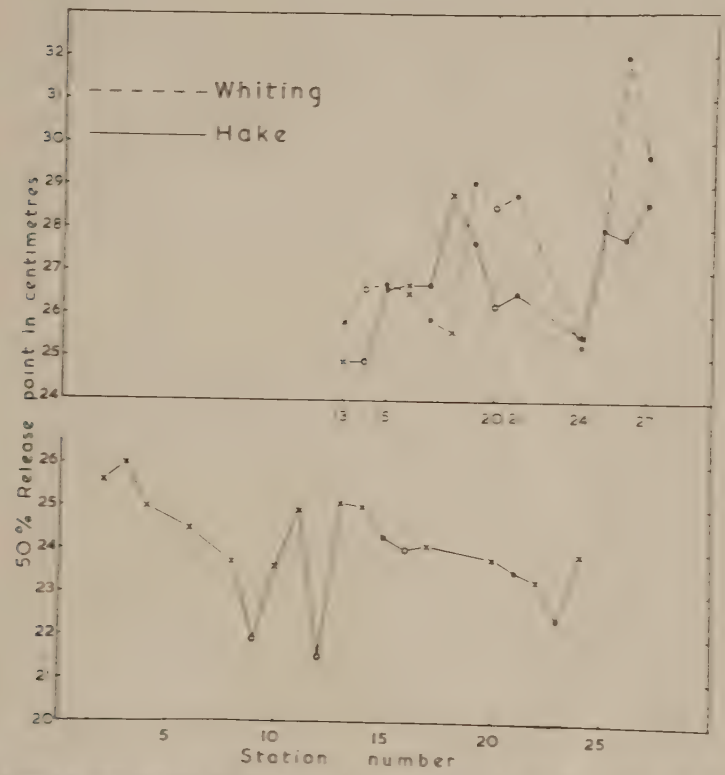

Figure 7. 50 % release lengths from data of individual hauls for hake and whiting with a 70 mm. mesh (above), and for whiting with a 74 mm. mesh (below). The apparent effect of length of haul in the latter series is shown.
× 3-hour haul. • 2-hour haul. ○ 1-hour haul.

catches were similar in each haul — ½–1 basket of mixed flatfish (plaice, witches, etc.), a few skate (less than 10), a few gurnards and ½–2 baskets of queens in the cod-end, and about a basket of mixed pout-whiting, smelt, and sofenettes in the cover. These catches were typical of all hauls in the 1955 experiment. The 50 % points for hake in the same hauls are 25·0 cm., 27·5 cm., and 27·3 cm.; these show much the same change between stations 24 and 25, but little or none between 25 and 26.

In Figure 7 the 50 % points obtained for hake and whiting at each station for which sufficient data are available have been plotted. Certain stations for which catches of one or other species were small have been combined; these are stations 4–6 (all 2-hour hauls) for the 77 mm. mesh, and stations 7 (a 1-hour haul) and 9 (a ½-hour haul) for the 74 mm. mesh. The mesh-size

(when measured) and the duration of the haul have also been shown. Considering first the data for the 74 mm. mesh obtained in the North Sea in December 1953, we find that the three outstandingly low 50% points were all obtained with short hauls (1 or 2 hours), and the 3 other short hauls all have 50% points at least no higher than adjacent 3-hour hauls. For the 70 mm. mesh, from observations in the Celtic Sea, in 1955, there seems little systematic difference between long and short hauls, except possibly for a high value for hake at station 18 (a 3-hour tow). The data from these experiments are therefore not sufficient to establish the hypothesis that length of haul is an important feature of mesh-selection, though the 1953 series gave suggestive results.

Comparison with other results

Hake

The results of the present experiments, and those in other published works, are given in Table 3. This gives the mesh-size (as measured by wedge-gauge when wet), type of twine, method of estimation, 50% release length, and the selection factor, i. e., the ratio of 50% point to mesh-size. DAVIS

Table 3

Summary of published selection factors for hake

Author	Mesh	Material	Method	50% Length	Factor
DAVIS (1935)	64	Double sisal	Covers	20·2	3·15
–	88	–	–	28·1	3·2
LETACONNOUX (1955)	60	Single hemp	Alt. haul	27	4·50
–	70	–	–	35	5·00
–	80	–	–	42	5·25
GULLAND (this paper)	77	Double sisal	Covers	29·7	3·86
–	70	–	–	26·1	3·73

stated his mesh-size in terms of spool-size — the two meshes being 8 cm. and 10 cm. on the spool. From his (appendix) Table XVI these appear to correspond to an internal measurement of 64 mm. and 88 mm. respectively. MARGETTS has suggested to me verbally that these figures should be further reduced by some 10% to allow for shrinkage. This would of course increase the factors correspondingly. The difference between the three sets of factors in Table 3 is very striking. The discrepancy between those derived in the present study and those of DAVIS may be fairly ascribed to differences in the manner of mesh measurement. The differences between the present observations and those of LETACONNOUX are greater; some may be due to differences in mesh — no actual measurements of the mesh while in use are given. Another source of difference is the materials used — the results of some other experiments — e. g., BOEREMA (1954) — suggest that a single-hemp cod-end will have the same selective action as a double-sisal cod-end 5–10 mm. greater. Some difference may also be due to the methods used. Of the two methods, that of alternate hauls, though in many ways the most satisfactory, is the more liable to errors. If in addition each mesh is used by a different

ship, as was done by LETACONNOUX, it is then possible that there are differences between the fishing powers of the ships, in the skill of their skippers, or in the grounds fished, if they are not fishing closely alongside, and the chance of errors is greatly increased. The last possibility is suggested by the different fishing depths given by LETACONNOUX (Annexe VIII). Certainly the great differences between the catches of hake above 50 cm. are very striking, being 0, 55, 12, and 14 for the 50, 60, 70, and 80 mm. meshes respectively, and if these differences, above the size of possible mesh-selection, are repeated within the selection range, then the estimated 50% selection point will be very inaccurate.

Whiting

The data for the mesh selection of whiting, particularly from recent years, are more extensive than for hake. The data from some recent experiments, together with those from the present series, are given in Table 4.

Table 4

Summary of some recently published selection factors for whiting

Author	Mesh	Material	Method	50% Length	Factor
FURNESTIN (1955)	73	Double manila	Covers	27·0	3·69
–	63	–	–	23·0	3·65
BOEREMA (1954)	67	Hemp	–	28·5	4·25
–	69	Manila	–	26·3	3·81
GULLAND (this paper) .	77	Double sisal	–	28·7	3·73
– .	70	–	–	27·1	3·87
– .	74	–	–	24·2	3·27

The figures for manila and sisal are in fair agreement, and the present values agree well with the average factor of 3·7 given by FURNESTIN.

The effect of mesh regulation on the hake fishery

Before estimating the effect of a larger mesh on the stocks of hake, it is worth comparing the escape of small fish through the larger meshes with the rejection by the fishermen of under-sized fish caught with the small mesh. The minimum legal size of hake is at present 30 cm., but in England there is little demand for the "needle" hake just larger than 30 cm., so that rejection by the fishermen is not very sharp, some considerably larger than 30 cm. being rejected. HICKLING (1933), Table XIII, has given data of this rejection at sea, based on 20,000 measurements on 6 commercial voyages. These are given in Table 5, in the form of the percentage rejected in each 5 cm. group, with the escape through the 77 mm. mesh as a comparison.

The table shows that for 30–39 cm. fish the escape through the meshes is less extensive than the present rejection rate, while the apparent higher value in the 40–44 cm. group is almost certainly only a sampling error, being based on 1 fish of 41 cm. caught in the cover. The true value, estimated from the smooth curve of Figure 1, would probably be less than 1%. In terms of

Table 5

Rejection of hake caught by commercial trawlers, compared with the escape through a 77 mm. cod-end

Length (cm.)	% Rejected (after HICKLING)	% Escaped (this experiment)
20–24	100	86·3
25–29	98·1	66·8
30–34	56·4	31·4
35–39	12·4	5·5
40–44	2·1	4·4
45–49	0·4	0

weight and value, hake of less than 40 cm. make up a very small fraction of the English catch and thus the loss to the fisherman of hake escaping through the meshes will be very small, and there will be the benefit of less unwanted fish and rubbish — and hence less crushing of the rather delicate hake — and also, probably, the increased catching power of the larger mesh. Against this must be balanced the increased proportion of marketable fish, mostly in the 40–50 cm. range, which become meshed and are damaged when being pulled out, whence their market value is reduced. The limited experience during the 1955 mesh experiment suggested that only a small percentage are thus meshed, and of these again only a few are damaged. Thus, even on the immediate results, the hake fisherman is not likely to be adversely affected by the larger mesh. The long-term effect, which is the fundamental purpose of mesh regulation, depends on the stock. With a factor of 3·8, and on the assumption that the use of a 75 mm. mesh would involve an increase of 10 mm. on the mesh in use in 1953, the 50% point is increased 3·8 cm.; i. e., the hake will be protected during the time they grow from 24·7 cm. to 28·5 cm. From HICKLING's (1933) data this corresponds to nearly half a year's growth. The mortality of the older fish may be obtained from his Table V, and is about 40% per year. The effect on the stocks of the cessation of fishing during the war suggests that much of this mortality must be due to fishing, say 30% per year.

Thus the protection from fishing for half a year would raise the number at the end, and hence ultimately the yield, by 15%. This estimate may be reduced by the extent to which the areas inhabited by young fish do not overlap the main areas fished, but a minimum estimate would seem to be 5–10%. A much faster growth-rate has been found by BAGENAL (1954) for the hake in the Clyde, but even if this applied to the hake of the Celtic Sea, thus reducing the period of protection by half, the estimate of increased sustained yield is little altered. To produce the observed length distribution the mortality must then be of the order of 80% per year, say 60% due to fishing, and again the first estimate of the increase is 15%.

This result suggests that an even larger mesh would be of ultimate benefit to the hake fishery. A 100 mm. mesh with a 50% point of 40 cm. would probably not be too large, though its introduction would have to be gradual, because it would release many fish in the 40–50 cm. group, which at present make up an important part of the catches. Because the price per pound of larger hake is considerably greater than that of small hake, the gain in value

to the English fishery through the use of a very large mesh would be greater than the estimates, which are based only on weight.

Summary

Two series of experiments, with 3 different cod-end meshes, all of double sisal, were carried out to determine the selection of hake and whiting by the meshes. The 50% release points when covers were used gave for North Sea whiting 24·2 cm. for a 74 mm. mesh, for Irish Sea whiting 28·7 cm. and 27·1 cm. for 77 mm. and 70 mm. meshes respectively, and for hake 29·7 cm. and 26·1 cm. for 77 mm. and 70 mm. meshes. The corresponding release factors, equal to the ratio of 50% point to mesh-size, were for whiting 3·27, 3·73, and 3·87, and for hake 3·86 and 3·73. The factors for whiting are in reasonable agreement with other recent experiments, but those for hake are much smaller than found by LETACONNOUX.

A short series of alternate hauls with large and small meshed cod-ends gave 50% points conforming with those found with covers, and tended to confirm the fact that the larger mesh gave bigger catches of fish above the selection range.

Variations in the selectivity from haul to haul are studied, and it is suggested that these are in part due to the length of haul — the longer the haul the more fish escape, and the higher the 50% point.

Girth measurements were made of many fish from cod-end and cover, and showed that girth was not a critical factor in the release of hake and whiting.

The introduction of a 75 mm. mesh in the hake fishery is studied, and it is estimated that the effect would be to give an average increase of 15% in the hake catches. A gradual increase of mesh to 100 mm. is suggested as ultimately bringing even further benefit.

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A Comparison of Mesh-Measuring Methods

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Introduction

Flat, parallel-sided or tapering, metal gauges of prescribed dimensions are at present in use by signatory countries for taking routine mesh measurements in the enforcement of the mesh regulations laid down by the International Fisheries Convention, 1946. The gauges are pushed by hand through the elongated mesh, and the criterion of a legal mesh is that the gauge must pass easily through when the mesh is wet. No force of insertion is prescribed but since the gauge must "pass easily", the force exerted must be small; in practice it will be variable.

It is well known by experienced handlers of those gauges that wide differences in the recorded measurements can be obtained by varying the force with which the gauge is inserted. Results of experiments designed to measure the increase in recorded mesh-size with increase in the force of insertion of the gauge are described by BOEREMA (1954) and VON BRANDT (1955). BOEREMA, working with wet manila and hemp cod-ends, showed an average increase of about 7 mm. in the mesh-size over a range of loads normal to the direction of measurement of between 1 and 10 Kg. (2–22 lb.), the increase in size being greatest over the range 1 to 4 Kg. (2–9 lb.). VON BRANDT worked with corresponding loads between 2 and 8 Kg. (4–18 lb.) on a wider range of materials and recorded larger changes in the measurements than did BOEREMA; moreover, he found that the increase in the recorded measurement for unit increase in load varied according to the twine used in the cod-end.

It is evident from these results, and from general experience in the use of these gauges, that there may be quite wide variations between the measurements taken by different operators. The question arises whether this variation can be reduced or eliminated by prescribing the standard force of insertion to be applied by enforcement officers in measuring cod-end meshes. This report deals with the results of some preliminary experiments which have been made at the Marine Laboratory, Aberdeen, to solve this problem.

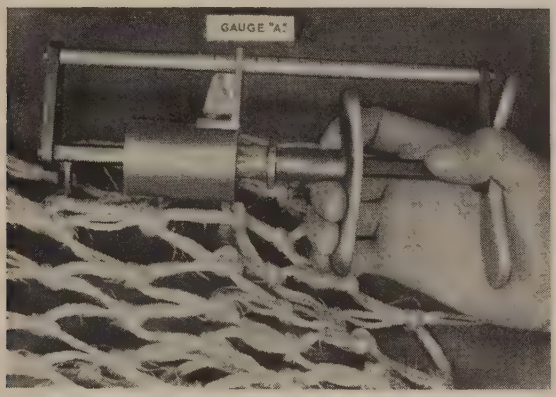


Figure 1. Scottish "pressure" gauge.

Comparative experiments with "pressure" and "non-pressure" gauges

The only practicable method of ensuring that successive mesh measurements are taken with an approximately constant force of insertion is to incorporate some form of spring into the gauge, so that a fixed compression of the spring provides a constant force between opposite knots of the mesh. Two such gauges have been used in these experiments, and the results obtained with them have been compared with those obtained with the wedge-shaped "non-pressure" gauges at present in use by Fishery Officers in the United Kingdom. The "pressure" gauges, A and B, are shown in Figures 1 and 2. Gauge A (Figure 1) was designed at the Laboratory and provides a *longitudinal force of about 8 lb. between opposite knots* — i. e., along the line of measurement of the mesh — when the spring is compressed by a fixed amount. This gauge has been used for measuring meshes at the Marine Laboratory since 1950 (see PARRISH, 1950). Gauge B (Figure 2) is the standard "pressure" gauge designed in the U.S.A. for mesh-measuring operations and prescribed in the mesh regulations recently imposed in certain of the I.C.N.A.F. areas. This gauge provides a force *normal to the direction of measurement* of up to 15 lb. The "non-pressure" gauges, C and D, used in the experiments are shown in Figure 3. Gauge C is the official 75 mm. gauge employed at Scottish ports in enforcing the mesh regulations for trawlers in the Convention area, and gauge D is a tapered metal gauge, 2 mm. thick, ranging from 60 to 120 mm. in width.

Comparative trials with each of the four gauges were conducted on two occasions in 1955. On each occasion a tarred manila cod-end (112's gauge twine) was used, and it was immersed in water for about one hour before each series of measurements was taken. Samples of about 50 measurements were taken with each of the gauges by each of three operators on each occasion, different operators being employed in the two experiments. The measurements were made on meshes selected at random, but to ensure that no bias arose from different operators choosing different parts of the cod-

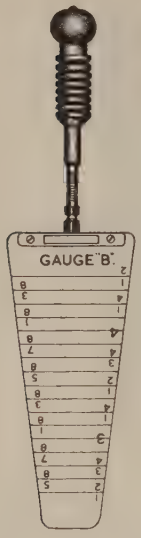
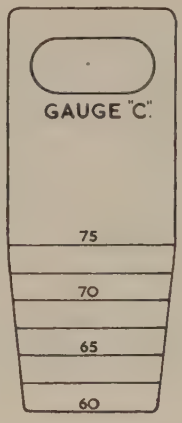


Figure 2. American "pressure" gauge.



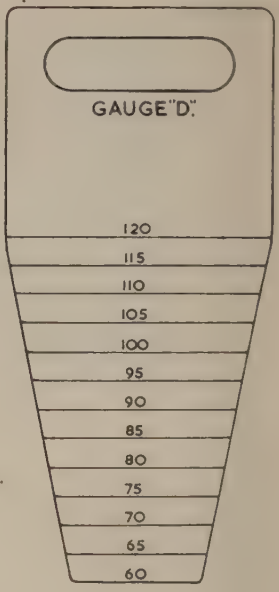


Figure 3. "Non-pressure" gauges.

end (between which there might be a difference in mesh-size) the cod-end was divided into sections of approximately the same number of meshes, and equal numbers of meshes were measured in each section. No measurements were taken in the vicinity of the selvage of the cod-ends. In the trials made with the "non-pressure" gauges each operator was instructed to use no greater pressure than he would consider compatible with the instructions issued to enforcement officers in Scotland. None of the readings taken by one operator was known by the others. The readings were taken with the cod-end laid out in the Laboratory yard, and for gauges B, C, and D the netting was pulled tight between the operator's hand and foot before the gauge was inserted. The order in which the gauges were used was chosen at random during each sequence of measurements.

The precision with which readings could be taken varied between the four gauges, due to differences in the scale intervals marked on them (see Figures 1–3). Gauge A is marked in 1 mm. intervals, gauge B in intervals of $\frac{1}{16}$ in., gauge C in intervals of 2·5 mm., and gauge D in intervals of 5 mm. In these experiments it was found that readings could not be taken with confidence with gauges B, C, and D at intervals smaller than half the marked scale interval, so that readings were recorded for them in intervals of $\frac{1}{32}$ in. (1·58 mm.), 1·25 mm., and 2·5 mm., respectively. Measurements with gauge A were taken to the nearest millimetre.

Table 1

Mesh measurements recorded by three operators with "pressure" and "non-pressure" gauges in experiment 1

Gauge A				Gauge B[1]			
Mesh-size mm.	Operator 1	2	3	Mesh-size mm.	Operator 1	2	3
64	–	–	–	–	–	–	–
65	3	3	–	–	–	–	–
66	1	1	1	–	–	–	–
67	7	5	3	66·8	1	1	–
68	15	10	9	68·3	2	2	–
69	11	7	12	–	–	–	–
70	8	9	11	70·0	11	8	8
71	7	5	4	71·3	13	11	9
72	5	7	8	–	–	–	–
73	2	2	1	73·2	17	16	25
74	1	1	2	74·7	2	12	3
75	–	1	–	–	–	–	–
76	–	–	–	76·2	3	–	10
77	–	–	–	77·7	1	–	–
78	–	–	–	–	–	–	–
79	–	–	–	–	–	–	–
Mean mesh-size (mm.)	69·10	69·47	69·76		71·95	72·23	72·96
Variance (mm.)	4·02	5·22	3·30		4·16	3·51	3·50

Gauge C[2]				Gauge D			
Mesh-size mm.	Operator 1	2	3	Mesh-size mm.	Operator 1	2	3
–	–	–	–	–	–	–	–
65·0	–	–	–	65·0	–	–	1
66·25	1	–	–	–	–	–	–
67·5	1	3	4	67·5	5	1	6
68·75	–	1	2	–	–	–	–
–	–	–	–	–	–	–	–
70·0	30	16	24	70·0	34	32	30
71·25	2	2	10	–	–	–	–
72·5	13	16	11	72·5	10	7	8
73·75	–	6	1	–	–	–	–
–	–	–	–	–	–	–	–
75·0	3	3	1	75·0	7	12	5
76·25	1	3	1	–	–	–	–
–	–	–	–	77·5	–	–	–
–	–	–	–				–
–	–	–	–			–	–
–	–	–	–	80·0	–	1	–
Mean mesh-size (mm.)	70·98	71·80	70·79		70·85	71·60	70·50
Variance (mm.)	3·39	4·86	2·97		4·15	6·03	4·59

[1]) Readings were taken in inches and converted to millimetres.
[2]) Meshes recorded as greater than 75 mm. in size were grouped in next size interval.

Table 2

Mesh measurements recorded by three operators with "pressure" and "non-pressure" gauges in experiment 2

Gauge A				Gauge B[1]			
Mesh-size mm.	Operator 1	2	3	Mesh-size mm.	Operator 1	2	3
60	–	–	–	–	–	–	–
61	–	–	–	–	–	–	–
62	–	–	–	–	–	–	–
63	–	1	–	–	–	–	–
64	1	1	1	–	–	–	–
65	3	1	2	65·0	1	–	–
66	4	2	3	–	–	–	–
67	9	5	5	66·8	1	5	–
68	10	4	8	68·3	2	2	1
69	6	7	9	–	–	–	–
70	7	9	11	70·0	7	18	8
71	5	10	4	71·3	11	9	12
72	1	7	3	–	–	–	–
73	1	3	2	73·2	18	12	20
74	3	–	1	74·7	5	3	3
75	–	–	1	–	–	–	–
76	–	–	–	76·2	4	1	5
77	–	–	–	77·7	–	–	1
78	–	–	–	–	–	–	–
79	–	–	–	79·5	1	–	–
Mean mesh-size (mm.)	68·64	69·54	69·14		72·29	70·93	72·55
Variance (mm.)	5·63	5·39	5·27		5·11	4·16	5·21

Gauge C[2]				Gauge D			
Mesh-size mm.	Operator 1	2	3	Mesh-size mm.	Operator 1	2	3
–	–	–	–	60·0	1	–	–
–	–	–	–	–	–	–	–
–	–	–	–	62·5	1	–	–
–	–	–	–	–	–	–	–
–	–	–	–	–	–		
65·0	4	–	–	65·0	10	–	–
66·25	2	–	–	–	–	–	–
67·5	7	2	3	67·5	14	–	4
68·75	3	6	3	–	–	–	–
–	–	–	–		–	–	–
70·0	16	10	14	70·0	15	25	33
71·25	7	7	12	–	–	–	–
72·5	8	13	12	72·5	7	12	8
73·75	1	5	3	–	–	–	–
–	–	–	–		–		–
75·0	1	4	1	75·0	2	12	5
76·25	1	3	2	–	–	–	–
–	–	–	–	77·5	–	1	–
Mean mesh-size (mm.)	69·90	71·72	71·25		68·50	71·95	70·70
Variance (mm.)	6·43	5·16	3·76		9·69	4·92	3·58

[1]) Readings were taken in inches and converted to millimetres.
[2]) Meshes recorded as greater than 75 mm were grouped in next size interval.

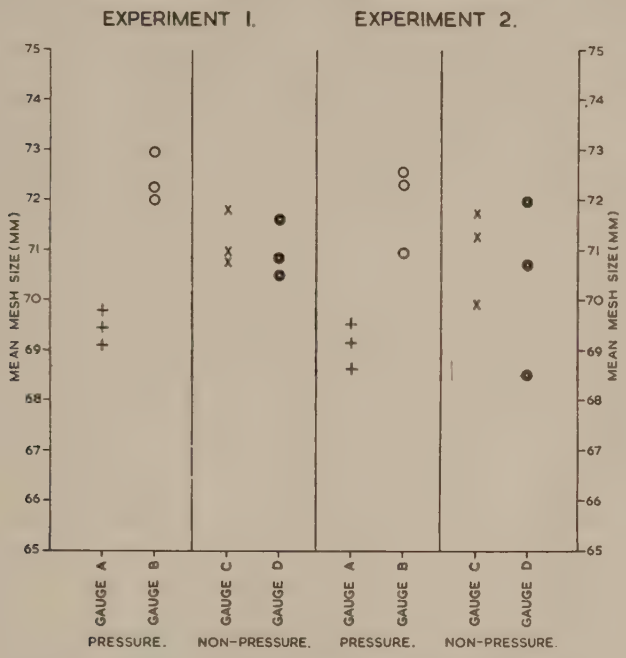


Figure 4. Mean mesh-sizes for different gauges and operators.

Results

The measurements taken by each operator with the four gauges in the two experiments are given in Tables 1 and 2, and the mean mesh-sizes determined from each series are plotted in Figure 4.

The results of greatest relevance to the present enquiry which are revealed by these data are as follows:—

(1) The mean mesh-sizes determined from the measurements taken with different gauges showed wide variation. In particular the readings taken with "pressure" gauge A were consistently lower than those taken with the "pressure" gauge B and, except for one series of readings with gauge D, also with those taken with the two "non-pressure" gauges.

(2) The means recorded by the three operators using the same gauge differed significantly for all gauges except gauge A.

(3) The differences between the means for each series of readings taken with the same gauge were greater for the "non-pressure" gauges than for the two "pressure" gauges. Individual variation was particularly marked with gauge D in experiment 2.

(4) Greater operator differences were recorded with "pressure" gauge B than with "pressure" gauge A in both experiments.

(5) There was no marked difference between the range and scatter of the

readings taken with the "pressure" and "non-pressure" gauges in experiment 1, but in experiment 2 there were marked differences between the range of values recorded by the three operators with the "non-pressure" gauge D.

These confirm earlier results (see Parrish, 1950) and support the general expectation that the provision of a tension device in the measuring gauge reduces the "between-operator" differences in mesh measurements and so ensures greater uniformity in the application of the enforcement measures under the mesh regulations. This is particularly so if the design of the gauge is such that a standard force is applied *along* the line of measurement, i. e., between the knots, as in gauge A, and not *normal* to the direction of measurement, as in gauge B. The results of these experiments show that the "between-operator" differences for gauge B, although smaller than those with the "non-pressure" gauges, were significant in both experiments. This difference in precision between the two types of "pressure" gauge has also been noticed during the course of general mesh-measuring work. The reason for this difference in precision is not easy to determine, and it is likely that it is due to the cumulative effect of a number of small weaknesses inherent in the method of measuring with a tapered gauge.

The correct pressure to apply

While the results of these experiments show that the overall precision of mesh measuring is increased by the use of some form of "pressure" gauge, it is evident that the two pressure gauges used in these experiments gave rise to systematic differences between the average mesh-sizes recorded for the same cod-end; on the average, gauge B gave means about 3 mm. higher than gauge A. Mean differences of this magnitude were also recorded when the two gauges were subsequently used in taking cod-end measurements on board ship.

This difference in performance is undoubtedly due in part to the different longitudinal forces exerted between the knots by the two gauges. Unfortunately, it is difficult to determine the exact force exerted by the tapered gauge along the line of measurement. It will differ from measurement to measurement according to the way in which the netting is held when measurements are taken, and the recorded measurement will also depend on the way in which the gauge is inserted into the mesh. In other words, it does not provide a constant pressure, as it is possible for the pressure to be influenced by the operator. In general, however, it is likely that the horizontal force exerted with gauge B in these experiments was rather higher than with gauge A.

It is of importance to consider what is the correct force to apply in practice. This can only be determined after some rigid definition has been laid down of the dimension of which a measurement is required. At present there is no precise definition for the European mesh regulations; all that is stated is that the measurements should be taken when the mesh is "stretched diagonally lengthwise". While this should ensure that measurements are always taken with the twine joining the knots fully straightened, there is still the possibility that they may be taken under a force which also tightens the

knots or stretches the twine elastically (the latter is more likely to be experienced with cotton and nylon twines than with coarse twines such as manila and sisal).

Three alternative standards for adoption in routine mesh-measuring may be considered.

(1) The force should be the same as that to which the meshes are subjected during fishing operations.

(2) A maximum force should be applied above which there is no further increase in recorded mesh-size.

(3) The force should be such that the twine is straight longitudinally but there is no tightening of knots or elastic stretching of the twine.

Of these alternatives the first might appear the most logical since with the force described the dimension measured would bear the closest relationship to the size of the mesh during fishing operations. However, at present very little is known about the loads on the meshes of the cod-end during fishing operations (see VON BRANDT, 1955, and SCHÄRFE, 1955), and they will undoubtedly vary between very wide limits according to the speed of towing and the size of the catch. Furthermore, they will vary throughout the course of a single haul and will change along the length of a cod-end. Therefore the choice of a standard on this basis is not possible at present and could at the best be only the average of a wide range of values. It is more practicable that the choice of standards should be made from the second and third alternatives. Both standards can be defined within reasonable limits for each type of twine used in trawls and seines by simple controlled experiments. Of these two we consider the third alternative the more appropriate standard to adopt in practice since measurements taken on the basis of the second may be associated with elastic stretching and severe knot-tightening and distortion (the degree of elastic stretching will probably be small for some twines, but large for others, e. g., nylon). The most difficult problem if the third alternative were adopted would be to determine the standard force for each type of twine, as the experimenter must judge when the twine between the knots is tight but not stretched. However, a close examination of the mesh under varying loads and a study of the rate of change in observed mesh-size with increase in load permit an estimate of the standard force to be made (BOEREMA, 1954).

As VON BRANDT (1955) points out, it is unlikely that a standard force can be chosen which will be applicable for all types of twine, and it may be that there must be separate standards for the twines used in deep-sea trawls, seines, and light trawls, and for nylon and perlon twines. The present experiments have been made exclusively on tarred manila double-braided trawl cod-ends and the standard which we consider satisfies the third alternative for this type of twine is a longitudinal force of 6–8 lb., which is approximately the same as that developed by gauge A. The equivalent downward force for a tapered gauge will of course depend on the angle of taper.

Summary and Conclusions

Experiments have been made with "pressure" and "non-pressure" mesh-measuring gauges to compare the precision with which they can be used. Two different types of "pressure" gauge were used, one giving a constant longitudinal force along the mesh directly, and the other giving a constant force normal to the direction of measurement. The "non-pressure" gauges were tapered metal gauges as used in Scotland in routine mesh-measuring at the fishing ports.

The differences between the mean mesh-sizes recorded for the same cod-end by different operators were smaller for the "pressure" than for the "non-pressure" gauges, and the greatest consistency was obtained with the gauge developing a constant longitudinal pressure directly.

Significant differences were recorded between the mean sizes obtained with the two types of "pressure" gauge, and these are discussed in the light of the standard of measurement required.

It is concluded that for manila and sisal twines, as used in deep-sea trawl cod-ends, a longitudinal force of 6–8 lb. is an appropriate standard.

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Reviews

K. F. Bowden. "Physical Oceanography of the Irish Sea". Min. Agric. Fish., Fish. Invest., Ser. 2, **18,** 8, pp. 1–67, 17 figs. London, 1955.

Die Arbeit stützt sich auf die Beobachtungen und Untersuchungen, die seit 1900 auf dem Gebiet der physikalischen Ozeanographie in der Irischen See ausgeführt worden sind. Sie befasst sich mit der mittleren Verteilung der Wassertemperatur, bezogen auf den Zeitraum 1903–31, wie sie von PROUDMAN, LEWIS, und DENNIS bestimmt wurde; und zwar für die Monate Februar, Mai, August, und November, und für das ganze Jahr. Sie erweitert diese Untersuchung auf Darstellungen der Temperatur am Meeresboden und der Temperaturschwankungen an der Oberfläche im Jahres-, Tages-, und Gezeitenrhythmus, sowie der langjährigen Temperaturschwankungen. Eine entsprechende Untersuchung, die sich auf die Originalbeobachtungen stützt, erfährt der Salzgehalt der Irischen See. Jedes der beiden Kapitel über Temperatur und Salzgehalt der Irischen See wird ergänzt durch Untersuchungen des Wärme- und Wasserhaushaltes, soweit sie Temperatur und Salzgehalt beeinflussen. Dabei können u. a. die mittleren Jahresschwankungen des Salzgehaltes von $0{\cdot}057^0/_{00}$ mit dem Maximum im März und dem Minimum im September nahezu vollständig aus dem Jahresgang von Verdunstung und Niederschlag erklärt werden.

Die Restströme werden diskutiert, die aus direkten Strommessungen, aus Flaschenposten, aus Temperatur- und Salzgehaltsverteilung, aus der Kontinuitätsbedingung für die Salzmenge und mittels der dynamischen Methode abgeleitet wurden. Abschliessend werden die Gezeiten- und Gezeitenströme behandelt. Ein ausführliches Literaturverzeichnis ergänzt die Arbeit.

Diese kurzen Hinweise auf den Inhalt dieser Arbeit mögen zeigen, dass hier der Versuch einer Monographie eines Meeres vorliegt und zwar vom physikalischen ozeanographischen Standpunkt, wie ihn in dieser Vielseitigkeit bisher weder die Irische See noch sonst ein Randmeer erfahren hat. Es würde sicher von vielen Seiten begrüsst werden, wenn auch andere Meere, nachdem vielfach systematische Beobachtungen über ein halbes Jahrhundert vorliegen, eine ähnlich zusammenfassende Bearbeitung erführen.

G. Dietrich.

Ch. Cox and **W. Munk.** "Statistics of the Sea Surface derived from Sun Glitter". Journ. Mar. Res., **13,** 2, pp. 198–227, 7 figs. New Haven, Conn., 1954.

In this paper Cox and Munk describe an investigation of the steepness ratio (ratio of height to length) of sea waves. The method used is a photometric one, depending on the reflexion of the sun by the waves. If the sea were smooth, the image would only be observed along the plane of incidence where the angle of reflexion is equal to the angle of incidence. This effect is blurred by the effect of the waves which present surfaces other than horizontal to the incident light, and the degree of blurring — or "glitter pattern", as it is called — gives a measure of the distribution of surface slopes. The experiments were conducted near Hawaii during September 1951.

The authors investigate the slope distribution along and at right angles to the wind direction, with a normal sea surface and with one covered with oil. It is found in each case that σ^2, the mean square slope, falls on a line which indicates a linear increase with the wind speed, the rate for the normal surface being about five times that for the oily surface.

The relation between the slope distribution and spectrum and the spectrum of surface elevation is deduced very elegantly.

The authors proceed to compare their results with the wave spectrum formula proposed by G. Neumann and by the reviewer. The Neumann spectrum gives a formal agreement, although the constant is five times less than the one obtained by the authors for normal sea surface. The Darbyshire formula shows that the slope distribution should decrease with the wind speed. The authors' comments and deductions about this formula are, however, very much open to question, and to follow the argument it would be better to quote the paper more or less fully on this point:—

If T_T is the energy contained in a unit interval of wave period, then the Darbyshire spectrum formula gives

$$T_T = c' T^2 \, exp \, [- 2{\cdot}0 \, (gT/U - 4{\cdot}6)^2]$$

where $c' = 22$ cm^2. sec.$^{-3}$, and U gradient wind speed $= 1{\cdot}5\, W$ where W is the masthead wind speed. On integration this formula gives a mean square height

$$< z^2 > = 6{\cdot}4 \cdot 10^{-5} \text{ cm.}^{-1} \text{ sec.}^3 \; U^3.$$

This formula is based on observations at Perranporth, Cornwall, with a wave-pressure recorder laid at a depth of 50 ft. so that waves of 5-sec. period or less were too much attenuated to be recorded satisfactorily.

The Neumann spectrum is based on visual observations and is

$$T_T = \tfrac{1}{8} c \, (gT^2/2\pi) \, exp \, [- 2 \, (gT/2\pi W)^2]$$

where W is the wind speed and $c = 0{\cdot}827 \cdot 10^{-3}$ sec.$^{-1}$. The mean square elevation due to this formula is $3{\cdot}2 \cdot 10^{-12}$ cm.$^{-3}$ sec.^{5} W^5.

If Darbyshire's data are fitted to Neumann's formula, they indicate a value of c which is $0{\cdot}3 \cdot 10^{-3}$ sec.$^{-1}$, compared with Neumann's value of $0{\cdot}8 \cdot 10^{-3}$. It is suggested that the value of c is small because the energy of waves lower than 5-sec. period has been ignored.

Taking NEUMANN's formula and integrating $\int_{T_1}^{\infty} T_T$ where T_1 is the cut-off period taken to be 6 sec., and equating this value to the mean square heights given by DARBYSHIRE's data, one obtains a value of the constant $c = 2 \cdot 10^{-3}$ sec.$^{-1}$ compared with the original value of $0{\cdot}3 \cdot 10^{-3}$ sec.$^{-1}$.

It can now be concluded that about five-sixths of the wave energy must be contained by waves of less than 6-sec. period and that DARBYSHIRE's spectrum only covers one-sixth of the wave energy.

The reviewer has several objections to these deductions. One objection is that if the authors had followed up their argument and substituted the new value of $c = 2 \cdot 10^{-3}$ in the NEUMANN formula for the mean square height, they would have obtained the formula

$$< z^2 > = 8 \cdot 10^{-12} W^5.$$

Now with a wind speed of 50 knots = 2500 cm./sec., one obtains a value of

$$< z^2 > = 80 \cdot 10^4 \text{ cm.}^2$$

This gives $< z > = 900$ cm. = 9 m. This corresponds to a crest-to-trough height of 18 m. Furthermore, it can be shown[1]) that the maximum height can be as much as twice the mean square height, so that a surface wind speed of 50 knots gives waves as high as 36 m. (or 120 feet) at Perranporth. Wind speeds of 50 knots are not uncommon near the coast of Cornwall but waves of 120 feet height have not yet been observed.

A second objection is the statement that five-sixths of the wave energy is contained by waves of less than 6-seconds period. The wind speed does not appear to come into this factor, so presumably this is supposed to be true at all wind speeds. Methods of recording surface waves directly have recently been developed, and surface waves were recorded and analysed at Eastbourne on 24. March 1955 at 4·30 p.m., when a wind of Force 5, direction W.N.W., was blowing. Although these conditions — a not too strong wind, and a limited fetch — tend to favour shorter-period waves, the proportion of energy contained by waves of more than 6-seconds period was well over half the total energy.

The reviewer has himself realized that the spectrum formula proposed in 1952 (Proc. Roy. Soc., A, **215,** p. 299) was imperfect and amended it in Proc. Roy. Soc., A, 1955, **230,** p. 560. The imperfections were not those suggested by COX and MUNK but rather that conditions near Cornwall, where the effect of tides and tidal streams is very pronounced, are not typical of the ocean as a whole. Since 1953, waves have been recorded by O.W.S. "Weather Explorer", using a shipborne wave-recorder designed by the National Institute of Oceanography. A new spectrum formula based on these ocean records was proposed, and with COX and MUNK's terminology it is

$$64{\cdot}0 \cdot 10^{-6} \text{ cm. sec.}^{-4} (U^{\frac{1}{2}} - 3{\cdot}06\, T)^2\, T^5.$$

[1]) See N. F. BARBER ("Ocean waves and swell". Inst. Civil Engineers, Marine and Waterways Div. London 1950) and M. S. LONGUET-HIGGINS (Journ. Mar. Res., **11,** p. 245. New Haven, Conn., 1952).

This formula applies to long fetches of over 100 to 200 miles. Work extending the formula to short fetches has not yet been published, but it shows that this spectrum formula is the limiting case of

$$64{\cdot}0 \cdot 10^{-6}\, y\,(U^{\frac{1}{2}} - 3{\cdot}06T/y)\,(T/y)^5$$

where y approaches 1 as x, the fetch, approaches ∞. If the fetch is in nautical miles, then y is approximately given by $1 - (exp - 0{\cdot}15\, x^{\frac{1}{2}})$.

An expression for σ^2 can be derived from this formula

$$9{\cdot}1 \cdot 10^{-10}\, U^2/y^2,\ U \text{ cm./sec.}$$

or, in terms of the masthead wind W,

$$\sigma^2 = 2{\cdot}04{\cdot}10^{-9} \text{ cm.}^{-2} \text{ sec.}^{-2}\, W^2/y^2$$

Cox and Munk obtain

$$\sigma^2 = 5{\cdot}12 \cdot 10^{-5}\, W \pm {\cdot}004$$

by the method of least squares, but a simple regression analysis of the points applying to a normal sea surface in the third section of their Figure 5 shows that

$$\sigma^2 = 4{\cdot}3 \cdot 10^{-5}\, W + 0{\cdot}009$$

with a correlation coefficient of 0·91. A square law

$$\sigma^2 = 3{\cdot}2 \cdot 10^{-8}\, W^2 + 0{\cdot}0178$$

can also be fitted and this has a correlation coefficient of 0·92. The square law would thus appear to be just as probable as the linear law and it affords a better means of comparison with the formula deduced above. The mean square slopes given by Cox and Munk are about 15 times higher than those given by the formula above for long fetches, but they become equivalent when $1/y^2 = 15$, which corresponds to a fetch of about 4 miles.

A scrutiny of the latitudes and longitudes of the positions of recording given in the paper reveals that most of the measurements were taken at a point about 5 to 6 miles equidistant from three islands and the wind was blowing offshore from one island or another during almost all the measurements. Cox and Munk's results are therefore consistent with what would be predicted from the new spectrum formula, the large zero constant being no doubt due to the incidence of swell from the gap between the islands.

To sum up, this method of analysis of glitter patterns is clearly a very useful way to prosecute wave research, particularly two-dimensional wave propagation, and is capable of giving good results if interpreted correctly.

J. D.

R. Fukai. "Critical studies on the analytical methods for minor chemical constituents in sea water". Part I. "On the estimation of phosphate-phosphorus". Journ. Oceanogr. Soc. Japan, **10**, 3, pp. 112–120, 5 figs. Tokyo, 1954.
Part II. "Some notes on the determination of silicate". Ibid., **10**, 4, pp. 201–8, 6 figs. Tokyo, 1954.

In these two publications R. FUKAI studies the determination of phosphate-phosphorus and the estimation of silicate in sea water. Both methods involve colorimetric determinations in a Beckman quartz spectrophotometer.

Phosphate-phosphorus was estimated by the DENIGÈS-ATKINS method. The maximum light absorption of the molybdenum complex formed at different wave-lengths was studied in various solutions of sodium chloride and in sea water. The maximum light absorption in any medium was found at 700 mμ, with Beer's law valid up to 100 mg./m^3. Stannous chloride was used as reducing reagent, and the author proved experimentally that the addition of 3–5 mg. Sn per 100 ml. sample was sufficient for spectrophotometric analysis on a semi-micro scale, that is, with samples of 5–10 ml. The correction for the salt effect was found to be 1·18, and the time for colour development after the addition of the reagents was studied. It was recognized that the time for development of maximum colour tended to increase with falling temperature, and it is recommended that the samples be allowed to stand for 5–15 minutes.

As a result of his studies, the author developed an analytical procedure not very different from the old one, but it is an advantage that the procedure accords with modern methods for colorimetric determination.

In the second paper R. FUKAI deals with the analytical method for determinating silicate. A wave-length of 380 mμ was found to be the most appropriate for the optical measurements of the yellow colour obtained. Beer's law is valid for the estimation of the coloured silicate complex up to a concentration of 10 mg./l. Both picric acid and potassium chromate have previously been used as standard solutions, and of the two the author prefers a solution of potassium chromate. The correction for salts above $5^0/_{00}$ was determined as 1·09 for spectrophotometric measurements at 380 mμ, and 1·18 for visual comparison. If glass bottles are used for the water samples, it is necessary to coat the interior of the bottles with paraffin.

This paper appeared before that by J. R. MULLIN and J. P. RILEY in which a new method of silicate analysis is described. This method will perhaps be the method mainly used in the future.

E. F.

J. B. Mullin and **J. P. Riley.** "The spectrophotometric determination of nitrate in natural waters, with particular reference to sea-water." Anal. Chim. Acta, **12,** 5, pp. 464–480, 2 figs. New York & Amsterdam, 1955.

The authors deal with the problem of finding a specific method for the determination of nitrate in sea water. Since nitrite can be very accurately and simply determined by the GRIESS-ILOSVAY method, a method for the

determination of nitrate is by reduction to nitrite and determination in this form. Methods which follow this principle are first described. One in which powdered zinc and acetic acid are used as reducing agent does not seem to be suitable for the determination of nitrate in sea water. In another, the nitrate is reduced by stirring granulated metallic zinc with the sea water while heating on a boiling water bath for 1·5 minutes; but this method does not give a very high yield of nitrite, as the nitrite is itself reduced to a certain extent. The analytical method for determining nitrite is so extraordinarily sensitive, however, that this method has been developed for the determination of nitrate in sea water containing between 20 and 500 mg. NO_3-N per cubic metre.

If a reducing reagent could be found which reduced the nitrate without reducing the nitrite, the sensitivity of the method could be greatly increased. When studying the reduction of nitrate by various reducing agents, MULLIN and RILEY discovered that hydrazine in alkaline solution in the presence of small numbers of copper ions yielded nitrite from nitrate in almost precisely the right quantity. Since the reaction worked only in a very narrow pH range, between 9 and 9·5, it was necessary to have a suitable buffer substance, and for this sodium phenate was chosen.

The catalytic effect of varying amounts of copper on the reduction process was investigated, and the reduction time and the effects of variations in the hydrazine sulphate concentration were studied, together with the interference caused by foreign ions and nitrite. The influence of temperature and salts was determined.

From the results of these studies, an analytical procedure was worked out; the procedure is briefly described.

The reduction time is 24 hours, which is fairly long compared with the 1·5 minutes required in the old method. The process involves setting aside the samples in the dark after the reagents have been added. The method will detect ca. 0·3 mg.NO_3-N per cubic metre and gives a standard deviation of ca. 2% in the range of 20–600 mg. per cubic metre. *E. F.*

R. Johnston. "Biologically active compounds in the sea". Journ. Mar. Biol. Assoc. U.K., **34,** 2, pp. 185–195. Cambridge, 1955.

Obwohl in den letzten Jahrzehnten gerade auf dem Gebiet der Meereschemie vor allem durch die Erforschung der anorganischen Minimumstoffe sehr grosse Fortschritte erzielt worden sind, welche zum Verständnis der biologischen Grundlagen der Lebensvorgänge im Meere wesentlich beigetragen haben, so ist man in bezug auf die im Meerwasser vorhandenen gelösten organischen Begleitstoffe, die zweifellos eine gleich wichtige Rolle spielen, über recht vereinzelte Anfangserfolge bisher kaum hinausgekommen.

Aus verschiedenen Versuchen biologischer Art, die in den letzten Jahrzehnten angestellt worden sind, ergibt sich immer deutlicher, dass organische Stoffwechselprodukte von Meeresorganismen in das Meerwasser abgegeben werden, welche bereits in geringen Konzentrationen wachstumsfördernde oder wachstumshemmende Einflüsse auf ihre Umwelt ausüben und somit indirekt auch für die Fischerei von wesentlichem Interesse sind. Über ihre

Natur und ihre Zusammensetzung ist man sich noch völlig im Unklaren. Der Grund hierfür liegt in erster Linie in den methodischen Schwierigkeiten, welche sich der Aufgabe entgegenstellen, solche in verhältnismässig geringen Konzentrationen vorliegenden Verbindungen im stark salzhaltigen Meerwasser anzureichern, die biologisch-aktiven und biologisch-inaktiven unter ihnen zu trennen und analytisch zu erfassen.

In neuester Zeit sind von R. JOHNSTON Versuche dieser Art in Angriff genommen worden mit dem Ziel, zunächst einmal zu klären, wie weit sich Seewasserproben auf Grund ihres Gehaltes an organischen Stoffen unterscheiden und ob diese ozeanische Mikroorganismen von sich aus beeinflussen können. Der praktisch einzuschlagende Weg lässt sich auf dreierlei Art durchführen:—

1) durch direkte Analyse der organischen Stoffe, welche man aus grossen Seewassermengen durch Extraktion, Adsorption, oder Dialyse gewinnen kann,

2) durch intensive Versuchsserien mit spezifischen Mikroorganismen in natürlichem und in mit Extrakten angereichertem Seewasser,

3) durch Isolierung der organischen Stoffwechselprodukte aus reinen Massenkulturen.

Eine besonders einfache Versuchsmethodik besteht in der Adsorption an geeigneten Adsorptionsmitteln, Behandlung mit verschiedenen Lösungsmitteln und anschliessender Trennung der Extrakte mittels Papierchromatographie.

Aus den Extrakten von Kohleadsorbaten liessen sich bisher vier verschiedene Stoffgemische isolieren: ein unlöslicher weisser Stoff, eine benzinlösliche gefärbte Substanz von carotinähnlicher Beschaffenheit, eine verhältnismässig grosse Menge einer wachs- und fettähnlichen bräunlichen Substanz, und ein gelblich-orangefarbiger Stoff, welcher im ultra-violetten Licht eine helle blaue Fluoreszenz zeigt. Einige andere Substanzen liessen sich bisher nicht identifizieren. Da die bisher gewonnenen Extrakte zu einer vollen organischen Analyse noch nicht ausreichten, wurden mit ihnen zunächst orientierende Vorversuche an 11 verschiedenen Phytoplanktonkulturen angesetzt. Hierbei liessen sich zwar Abweichungen fördernder und hemmender Art gegenüber der Blindprobe feststellen, engere Beziehungen systematischer Art waren jedoch aus dem ungleichförmigen Untersuchungsmaterial noch nicht abzulesen.

Als nächstliegende Aufgabe ist eine Verbesserung des Arbeitsganges und eine weitergehende Fraktionierung der Extrakte geplant, so dass man hoffen kann, der praktischen Schwierigkeiten, die der Lösung des vorliegenden Problems bisher hindernd im Wege standen, nach und nach Herr zu werden.

K. Kalle.

G. Thiel. "Über die Auswirkung der ablenkenden Kraft der Erdumdrehung auf die Wasserstände im Grossen Belt". Deutsche Hydrogr. Zeitschr., **7**, 5/6, S. 162–173, 6 Abb. Hamburg, 1954.

The connexion between current and water-level in long basins is a question which deserves special attention. There are without doubt cases in which

the direction and speed of the current at any moment, or the average current, may be estimated from sea-level observations made on gauges situated on opposite sides of the basin, or vice versa. The present paper, which deals with conditions in the Great Belt, is a valuable contribution to this question. A comparison is made between observed and theoretical values determined by considering the influence of the deflecting force of the earth's rotation. The data used cover four different periods of 6 to 9 weeks each, occurring during the years 1937 and 1938. The sea-level observations were made on the gauges at Korsør and Slipshavn, and the current measurements on board the lightship "Halsskov Rev", which is located nearby. A first inspection shows that the differences in sea-level at the two stations follow the variations in direction and speed of the current reasonably closely. Closer examination reveals, however, that on the average the sea-level differences caused by the effect of the Coriolis force reach only half the theoretical amount. This deviation is due principally to the effect of local topography. There are also other, less frequent deviations. The correspondence between theoretical and observed values is, for instance, less satisfactory in stormy weather, as the effect of the local wind may disturb the system.

E. Lisitzin.

K. Wyrtki. "Schwankungen im Wasserhaushalt der Ostsee". Deutsche Hydrogr. Zeitschr., **7**, 3/4, S. 91–129, 15 Abb. Hamburg, 1954.

The fluctuations of the water volume in the Baltic is a problem which interests hydrographers in most of the countries bordering on this sea. Its study is very complicated, however, since it requires close consideration of so many different factors. Moreover, annual and monthly averages are frequently too coarse to enable a satisfactory solution of the problem to be reached, and it is therefore sometimes necessary to consider characteristic cases separately. The author has used very comprehensive data for his research and his paper is illustrated with a considerable number of tables and figures. He takes into account climatological factors — freshwater influx into the Baltic, precipitation and evaporation — as well as the dynamical exchange of water with the North Sea through the Belts and the Sound. The water exchange is, of course, influenced by the prevailing winds and the sea-level differences, and both of these factors are studied carefully. The author notes that in months with a strong westerly component of the wind in the transition area, the outward water transport calculated from the freshwater effect and sea-level variations diminishes, and in October, when the west wind reaches one of its highest values, the transport changes direction. This shows the great importance of weather conditions upon water exchange. The author has also compared the calculated water transport with the observed current in the Belts and the Sound. The agreement is perhaps surprisingly good, although surface current only is taken into account. As regards its effect on water transport, sea-level in the Baltic seems sometimes to be more important than the wind. We find in the paper examples of cases with strong west wind, but with only a weak inflow, as a result of the high sea-level. In this connexion the author emphazises that the use of monthly averages, which combine a variety of different cases,

may give rise to considerable scatter in the observations and render it difficult to reach definite conclusions.

The author deals with long-period fluctuations of the water balance in the Baltic. He shows that there is a correspondence between the general trends of the different factors affecting water volume in the Baltic. One of the main results, for instance, is that changes in the atmospheric circulation during the 'thirties caused a distinct lessening of precipitation in the area around the Baltic, which was followed by a decrease in sea-level and in outflow of water.

The last part of the paper is devoted to the salt exchange between the North Sea and the Baltic. This is of especial interest, in view of the considerable salinity increase observed during recent years in this sea. The salt exchange in the years 1950 and 1951 has been studied by means of numerical data.

E. Lisitzin.

G. Wüst. "Gesetzmässige Wechselbeziehungen zwischen Ozean und Atmosphäre in der zonalen Verteilung von Oberflächensalzgehalt, Verdunstung und Niederschlag". Arch. f. Meteor., Geophys. u. Bioklim., Ser. A, **7**, S. 305–328, 5 Abb. Wien, 1954.

G. Wüst, W. Brogmus, und **E. Noodt.** "Die zonale Verteilung von Salzgehalt, Niederschlag, Verdunstung, Temperatur und Dichte an der Oberfläche der Ozeane". Kieler Meeresforsch., **10**, 2, S. 137–161, 3 Abb., 10 Tab. Kiel, 1954.

These two papers cover much the same subjects, and make use of the same basic data, so that they may be reviewed together.

At the Institut für Meereskunde in Kiel, the very extensive task has been carried out of computing new zonal mean values of surface temperature and salinity; in addition, the values for precipitation and evaporation have been critically examined. The number of observations has increased vastly since KRÜMMEL, in 1907, computed the mean values of temperature and salinity for 5-degree zones, so it seems fitting that a fresh calculation should now be made. The values also include marginal seas. Naturally, the temperature values need only small adjustment, as the number of temperature observations was already relatively large in KRÜMMEL's time. In fact, the deviations from KRÜMMEL's values are hardly significant. For salinity, however, the differences are in some cases considerable. Thus, for the lower latitudes in the Pacific, KRÜMMEL's values are generally too high. The mean surface salinity of all oceans, including marginal and polar seas, has been estimated at $34{\cdot}73^{0}{}_{00}$ as compared to KRÜMMEL's 34·49. As regards precipitation and evaporation, direct observation at sea is difficult, and to a great extent, one has to use more or less indirect methods. Systematic observations of precipitation, covering periods long enough to give trustworthy mean values, only exist for islands and coastal stations, and such observations are influenced by orographic effects that do not occur at sea. Furthermore, observations are entirely lacking for vast regions of the oceans. Under such circumstances, the results will be rather rough approximations. The author discusses these problems, and concludes that MÖLLER's charts of precipitation (1951) are

on the whole the most reliable source from which to compute the zonal mean values of precipitation. These charts are based on figures obtained by reducing the island and coastal measurements of precipitation by about 20%.

Evaporation is still more difficult to estimate. Several methods, mostly indirect, have been attempted by various investigators, and WÜST subjects them to thorough examination. In any case, a mean error of ± 15% in the zonal mean values must be expected.

Summing up the difference between evaporation and precipitation in one year over all the oceans, WÜST arrives at a value (27,200 km.^{3}/year) that is surprisingly close to the total run-off from the rivers of the earth (28,800 km.^{3}/year). However, this agreement must be considered as accidental, as the value is much less than the error in the determination of evaporation mentioned above.

The zonal values of salinity (S) and evaporation minus precipitation (V–N) are plotted in a (V–N)/S diagram, in the same manner as in WÜST's paper of 1936. When using salinity values for the open ocean only, one obtains a statistically satisfactory linear relationship between the variables, but, whereas in WÜST's 1936 paper all the points are grouped about one straight line, they now lie along two lines, one for the northern (to 10°N.) and one for the southern hemisphere. WÜST explains this as an expression of the difference in the distribution of land and sea in the two hemispheres.

There are, however, several factors other than the difference (V–N) which influence the salinity distribution of the ocean surface. The general circulation, for instance, accounts for a pronounced difference in salinity between the eastern and western sides of the oceans. According to WÜST, such differences are eliminated to a great extent by the averaging process and do not mask the linear relationship between (V–N) and S, which he sees as an expression of a global law concerning the interaction between ocean and atmosphere.

O. H. Sælen.

H. Postma. "Hydrography of the Dutch Wadden Sea". Arch. Néerland. Zool., **10,** 4, pp. 405–511, 55 figs. Leiden, 1954.

Before World War II the estuarine habitat received most attention from biologists interested in the adaptation of marine fauna and flora to its varying salinity and exposure characteristics. Since the war, however, greater attention has been paid to the rather complex circulation and productivity characteristics of the estuarine environment.

For the most part the individual papers have dealt with a certain facet of the environment, without consideration of the estuarine system as a unit. In this present paper, however, POSTMA rightly stresses the relation between water movement, transport of suspended matter, and the cycle of suspended organic matter. Throughout this excellent paper he never loses sight of their interdependence and we are indebted to him for a unifying approach to the subject of estuarine hydrology.

The author considers firstly the rate of exchange of Wadden Sea and North Sea waters. From earlier studies on the Wadden Sea (1950) he is able to demonstrate how horizontal mixing is strongly developed in this

region and why vertical salinity stratification is not greater than $1^0/_{00}$ in a depth of 15 m. (Fig. 11). Mean velocities of 60–70 cm. sec. (p. 25) compare with velocities of 50–60 cm. sec. for the James River[1]) but the velocity stratification of the latter is apparently absent in the Wadden Sea. This absence of vertical velocity differentiation leads the author to conclude that the fresh water is not transported as a surface layer but as part of a mixture which is of about the same composition near surface and bottom. This type of circulation is also a feature of many Australian estuaries[2]) and a similar vertical cellular circulation may also be effective in the Wadden Sea.

Considering the transport of fresh water through the Wadden Sea, the author is able, because of reliable gauging data on the Ijsselmeer, to make a reasonable calculation of the amount of fresh water exchanged with the North Sea per tidal cycle in order to maintain a given $Cl^0{}_{00}$ level in the Wadden Sea. This discharge amounts to some 8% of the total quantity of fresh water. By a modification of KETCHUM's[3]) tidal prism method, POSTMA makes a semi-interdependent check of the rate of freshwater transport per tidal cycle, and arrives at a figure which agrees fairly well with the above estimate.

Considering the mechanism for the maintenance of a given silt load in the Wadden Sea, POSTMA comes to the conclusion that vertical circulation, particularly associated with wind, is the major factor responsible for maintaining a silt load against the settling effects of gravity. By examination of all available data he is able to show that the average concentration of silt in the Wadden Sea is some 18 mg. l., compared with 6 mg. l. for the adjoining North Sea and 15 mg. l. for the Ijsselmeer. By calculation of silt transport he is able to show that the Ijsselmeer discharge of silt into the Wadden Sea is negligible. To account for the higher concentration of silt in the Wadden Sea, POSTMA suggests that the transport of silt per tidal cycle is not equal for both flood and ebb phases. This difference is largely brought about by the inertial properties of the suspended (compared with the dissolved) load of a water mass. This effect is also augmented by the decreasing gradient of velocities of water movement towards the inner region of the Wadden Sea, permitting sedimentation of some of the silt load to take place. POSTMA's interpretation means that far less silt is transported to the North Sea per tidal cycle than any dissolved substance associated with the 8% tidal discharge. It is conceivable that for some part of the year the silt cycle in the Wadden Sea is nearly a closed one with little contribution from either the North Sea or the Ijsselmeer.

Having considered the physical factors influencing circulation and transport of sediment in the Wadden Sea, the author considers the cycles of phosphorus and organic matter in the Wadden Sea. Because of the fairly high "silt" load of the Wadden Sea, POSTMA has been able to derive some

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[1]) D. W. PRITCHARD (1952). "Salinity distribution and circulation in the Chesapeake Bay estuarine system". Journ. Mar. Res., **11**, 2.

[2]) D. J. ROCHFORD (1951). "Studies in Australian Estuarine Hydrology. I. Introductory and comparative features". Australian Journ. Mar. Freshw. Res., **2**, 1.

[3]) B. H. KETCHUM (1951). "The exchange of fresh and salt waters in tidal estuaries". Journ. Mar. Res., **10**, pp. 18–38.

complete figures for its carbon, nitrogen, and phosphorus content. His C:N:P relation of 40:7·4:1 compares very well with that for living plankton. However, because of lack of geographical evidence POSTMA assumes that the distribution of organic matter parallels that of silt and accordingly concludes that the concentration of the former will be higher in the Wadden than the North Sea.

Turning now to a consideration of the phosphorus cycle, he dismisses, on somewhat indirect evidence, particulate inorganic phosphates as of any consequence in this cycle. But the method of phosphate estimation followed (p. 10) will include phosphates released from particulate inorganic phosphates by acidification, and in view of the high silt load of the Wadden Sea, this contribution might sometimes be a significant part of the inorganic phosphates. POSTMA considers that the direct exchange of phosphates between mud and water is not a factor of any importance in the development of the spring rise and autumn decrease in total P of the Wadden Sea water. His evidence in support of this conclusion is not very positive, however, and in the absence of any data on bottom O_2, pH, and redox conditions, we cannot dismiss the possibility that sorption exchange of phosphates from the bottom deposits into a boundary layer of water above the mud surface might not be the precursor to biological fixation of phosphate, which increases the total phosphorus content of the water. POSTMA, after a careful examination of all possible factors involved in the phosphorus cycle of the Wadden Sea, came to the rather important conclusion that the "gradient of dissolved phosphorus is probably caused by the activity of bottom animals combined with the decomposition of large amounts of organic matter accumulated both in the water and in the bottom of the Wadden Sea through the constant movement of suspended matter towards the interior".

Sometimes we tend to forget that the marine habitat is not a sterile one and that the fauna and flora, as in this case, can play a very significant role in what might be considered a purely chemical cycle.

Chlorophyll determinations were commenced in the Wadden Sea in April 1951, but unfortunately no phosphorus values are available after July 1951. No complete comparison between chlorophyll and phosphorus cycles can therefore be made, but it is interesting to note that the March-April rise in chlorophyll each year (Fig. 54) could be associated with the corresponding rise in previous years of particulate and dissolved organic P for the Wadden Sea as a whole (Fig. 53).

By using these chlorophyll figures and applying corrections for the percentage that is associated with detrital organic matter and therefore not capable of photosynthesis, POSTMA estimates the amount of "living" chlorophyll and from this the amount of phytoplankton in the Wadden Sea. Using a figure given by HARVEY for reproductive rate he calculates the production of organic matter by phytoplankton in the Wadden Sea, and concludes that the supply of organic matter from this production is of the same order as the supply by the North Sea to the Wadden Sea. This equality of organic production is a little unexpected and in view of the assumptions used in its derivation, a critical examination by future workers on the Wadden Sea of the ratio of "living" to "detrital" chlorophyll would seem

to be indicated. The technique of fluorescence microscopy as a method of differentiation of "living" chlorophyll could be considered as an appropriate technique. Some measurements of turnover rates of phytoplankton in the Wadden Sea should also be considered in any future continuation of this valuable study.

D. J. Rochford.

J. H. Fraser. "The plankton of the waters approaching the British Isles in 1953". Scottish Home Dept., Mar. Res., 1955, 1, pp. 1–12, 5 figs. Edinburgh, 1955.

According to the plankton and hydrographical data presented in *Annales Biologiques* for 1953, that year was one of unusual oceanic influence in the waters around the British Isles. In the present paper Dr. FRASER gives further details of the plankton communities of the incoming oceanic water masses.

Collections of plankton were made off the entrance to the English Channel on four sections from April to August 1953. Samples were taken in the Faroe-Shetland Channel from February to August, and in the northern North Sea during the periods January–April and September–December. The material also includes samples taken between Scotland (west of the Hebrides) and Iceland in June and August.

The author lists a number of organisms that can be used to distinguish the Lusitanian or Mediterranean fauna from that of the North Atlantic Drift. According to the author, the latter is usually characterized by the presence of cosmopolitan oceanic species, but without an appreciable representation of Lusitanian species. In 1953, however, the Lusitanian component was particularly marked.

The various plankton stations are plotted on charts, and figures indicate the numbers of Lusitanian and cosmopolitan species. In these charts the course of the Lusitanian species can be traced from off the west coast of Ireland, past the west of Scotland, and through the Faroe-Shetland Channel into the northern North Sea.

The inflow of oceanic water to the North Sea coincided with the failure of the herring fishery off Shetland. It was associated with a scarcity of crustaceous plankton and other organisms which are usually important as food for the herring, and the author suggests that this scarcity may have contributed to the failure of the herring fishery.

The paper also contains valuable information about Lusitanian species, of which many have not previously been reported from the North Atlantic.

Omission from the present paper of information about type of nets used, depth of hauls, etc. is to be regretted, but for the samples taken off the English Channel, these data can be found in a previous paper by the same author in Journ. Mar. Biol. Assoc. U.K., 33, 1954.

O. J. Ostvedt.

K. Demel and **Z. Mulicki.** "Studia ilościowe nad wydajnością biologiczna dna poludniowego Baltyku". ("Quantitative investigations on the biological bottom productivity of the South Baltic"). Rep. Sea Fish. Inst. Gdynia, 7, pp. 75–126. Warszawa, 1954.

This paper by the well known Polish authors, working at the Sea Fisheries Institute in Gdynia, provides a most important addition to our knowledge of the bottom fauna of the southern Baltic. It represents a continuation of previous Polish work aimed at investigating the productivity of the benthal fauna of Baltic waters.

The material was collected in the years 1948–1952 from some 220 stations, covering, in the east, the area between a line from the southern end of Gotland to a point somewhat east of the Gulf of Gdynia, and in the west, the waters between Scania and Rügen. Usually 3 samples, each of 0·10 m.^{2}, were taken with the Petersen grab at each station.

On the basis of this extensive material "isobenthals", i. e., areas of equal biomass (expressed as g./m.^{2}), were calculated for the most important bottom animals (*Macoma baltica*, *M. calcarea*, *Astarte borealis*, *Mytilus edulis*, *Mesidotea entomon*, *Pontoporeia affinis*, *P. femorata*, *Diastylis rathkei*, *Scoloplos armiger*, *Halicryptus spinulosus*); the results are presented in charts for each species. From the isobenthals the authors were able to calculate the total quantities of animals present; the value arrived at for *Mytilus* is about 3,400,000 tons; for the other species at most about 840,000 tons (*Macoma baltica*). The biomass of the whole benthos in the area, without *Mytilus*, amounts to about 1,360,000 tons. A chart of the isobenthals of the whole bottom fauna (without *Mytilus*) shows that the productivity (standing crop) varies very much from region to region. The districts with greatest biomass are found between Bornholm and Scania, between the east of Bornholm and the south of Öland, south of Gotland, and in the western part of the Gulf of Gdynia.

Two diagrams illustrate in a very instructive manner the quantity and composition of the bottom fauna along cross-sections through the Bornholm Basin and the Slupsk Channel (between the Bornholm Basin and the Gotland Deep); they also show the salinity, temperature, and oxygen, and the nature of the bottom deposits. A number of tables provide detailed information on the biomass in different depths of the subareas, as well as the original station data (together with temperature, salinity, oxygen, and bottom deposits for each station). The nomenclature used in these tables is in some cases antiquated or incorrect: for instance, *Membranipora membranacea* instead of *M. crustulenta*; *Gammarus locusta* (the recent revision of the Baltic *Gammarus* species has not been followed); *Idothea tricuspidata* is today named *I. baltica* (in addition *I. granulosa* and possibly *I. viridis* might occur in the material); the more usual name for *Neritina* today is *Theodoxus*.

On the basis of the results obtained, the food supply of the fish in the area is discussed and the relationship between the quantity of benthos present and the yield of the fishery is compared with that recorded for other marine areas.

Despite its mainly practical purpose the paper under review, based as it is on very extensive material from wide areas of the Baltic, gives much useful information of academic interest to the zoologist, especially as regards the occurrence and environment of the benthic animals of the southern Baltic. It is to be regretted that not all Baltic biologists will be able to read the paper in full, written as it is in Polish. It is because of the language difficulty that this review is perhaps not as detailed as it might have been.

S. Segerstråle.

B. Werner. "Über die Anatomie, die Entwicklung und Biologie des Veligers und der Veliconcha von *Crepidula fornicata* L. (Gastropoda Prosobranchia)." Helgoländer Wiss. Meeresunters., **5**, 2, S. 169–217, 22 Abb. List, 1955.

This quite lengthy and well illustrated monograph treats the all-important planktonic stage of one of the greatest pests of oyster beds as extensively as ERDMANN treated the corresponding stage of the oyster. The larvae, feeding on the organisms which happened to be present in the water, were hatched and reared with remarkable success in large laboratory tanks. They were studied alive for the most part, and also by a variety of histological methods. Many interesting points emerged, a few of which may be noted briefly.

The preceding incubation period was shown to last about three weeks, by inducing the lowest female of a chain to deposit her egg capsules on a glass plate, through which the course of development could be observed. Escape of the larvae on hatching was assisted by the mother lifting her shell, and followed by her rasping off and ejecting the empty egg capsules. The hatching larvae are typical gastropod veligers. They feed on unicellular green algae and flagellates, rather than on diatoms. The food organisms are collected by the large cilia of the two rings which border the velum, directed into the groove which lies between the rings, entangled in mucus, and conveyed to the mouth by the fine cilia which line the groove. Ciliary transport and rotation of food within the gut are described. Muscular contractions force the food from the stomach into the liver diverticula, where digestion takes place.

The larval heart, which lies immediately behind the velum, is equipped with anterior and posterior valves. These ensure that the blood is pumped posteriorly, from the velum, where it is presumably oxygenated, to the viscera, which are cut off from the surrounding water by the calcareous shell. The author's further suggestion, that the heart serves to fill the velum and foot with fluid during extrusion, seems more doubtful. Though it is convenient to refer to the velum as "expanding", it may not be altering in volume. It is capable of withdrawing again with such speed as to suggest that it merely changes in shape. The heart is described, moreover, as pumping fluid out of the velum, and the return circulation as being passive. Nevertheless, the larval heart is clearly linked functionally with the velum, gradually regressing towards the end of the planktonic life, when the definitive heart, pulsating independently, takes over the circulation.

The larval life lasts from 7 days at 19·5°C. to 14 days and perhaps longer under natural conditions. The shell length increases from ·33 to ·36 mm. at hatching, to ·59–·88 and occasionally to over 1 mm. at metamorphosis. The late larva or veliconcha is capable of both swimming and crawling, and shows advanced development. Five to eight large gill filaments draw a strong water current into the mantle cavity. Even the peculiarities of the adult shell are foreshadowed, the rudiment of the septum being formed by part of the mantle, which projects posteriorly when the animal is extended, and secretes shell material on the outside of the area of shell which faces the substratum during crawling. Around the margin of this area the posterior rim of the adult shell begins to form. The nerve ganglia and commissures are laid down. As in other veligers they appear later than the sense organs (eyes, statocysts, and tentacles) and long after torsion has finished, a fact with which many zoologists will be unfamiliar.

To sum up a detailed, but interesting, description, the rudiments of all the main adult organs, except the gonads, are developed by the end of the planktonic stage. The late veliger, which thus appears well equipped to test its environment from an adult viewpoint, usually spends one or two days alternately swimming and crawling, apparently searching for a place in which to embark upon its sedentary life. Eventually metamorphosis occurs, within a space of ½ to 1 hour. First the operculum is cast off, and then the velum. Some of the velum is lost, some appears to be eaten. In a day or two only its small size, immaturity, and active crawling distinguishes the animal from an adult.

E. W. Knight-Jones.

P. N. J. Chipperfield. "Observations on the breeding and settlement of *Mytilus edulis* (L.) in British waters". Journ. Mar. Biol. Assoc. U.K., **32,** 2, pp. 449–476, 8 figs. Cambridge, 1953.

It is remarkable how little is known regarding the breeding and settlement of the common edible molluscs, the cockle and the mussel. In the case of the mussel much of the published information is somewhat contradictory and although the present study makes a considerable contribution, it suffers from the drawback of relating, in the main, to the events of two years only, viz., 1946 and 1947. It will be recalled that there was intense frost in Britain in the early months of 1947, which is likely to have resulted in somewhat atypical spawning behaviour in that year. However, on the positive side, the paper presents evidence from widely scattered populations from Skye, Liverpool, Brixham, Brancaster, Conway, etc.

To assess the stage of gonad development, the thickness and colour of the mantle were recorded and microscopic smears were examined. Sections of the mantle were also prepared at times to fix the stages of development more exactly. Individuals in the samples were referred to one of five categories, viz., resting stage or neuter, onset of gametogenesis, unripe ova or sperm, ripe ova or sperm, and recently spent. These stages are described in detail and seem reasonably distinct and well marked, but the tables reveal certain apparent anomalies which presumably resulted from faulty diagnosis of the stage of development in a small proportion of the examples examined.

Ripening of the gonads was found to occur within a few weeks after the seasonal rise of sea temperature to 7°C. Actual spawning commenced in a period during which the mean sea temperature over the mussel beds was rising from 9·5° to about 12°C. Such conditions occurred during the years under investigation in late spring (mid-April to end of May) in all localities investigated, and spawning was usually completed very quickly, often in a fortnight and only occasionally extending over as long as 4 weeks. No evidence of periodic spawning was obtained.

As might be expected from comparison with detailed work done on the oyster, the duration of the pelagic life of the larvae appeared to be somewhat variable, averaging 3–4 weeks. The size at settlement also seems to vary, but it has since been shown by other workers that post-larval stages may be swept about by tides and currents for several weeks after initial settlement.

It is noted that heaviest settlement of spat occurs upon hydroid colonies, the rough or frayed surfaces of ropes and on rough shells. This is in accordance with Dutch experience, which indicates that a prior settlement of hydroids is an important factor in the settlement of mussels.

It was found that on floating structures mussels attached themselves most abundantly from the surface down to 2 feet and a similar distribution was observed on fixed vertical substrates. On the shore the greatest settlement was noted at or below low water of spring tides, but the observations were not extensive and experience on some major beds, e. g., in the Wash, is that heavy spatfalls may occur at higher levels.

Although this paper provides a useful extension of our knowledge of breeding and settlement in *Mytilus edulis*, the picture is still far from complete. More extended and much earlier settlement seasons have been observed in other years; for example, at Conway in 1940 the heaviest spat noticed for many years settled in late March and early April after an exceptionally cold winter.

H. A. Cole.

P. M. L. Tammes and **A. D. G. Dral.** "Observations on the straining of suspensions by mussels." Arch. Néerland. Zool., **11**, 1, pp. 87–112, 12 figs. Leiden, 1955.

In experiments with individual *Mytilus*, measurements were made of the rates at which water was pumped through the gills, and of the percentages of suspended material removed during the process, when the water contained suspensions of various particle sizes.

To allow collection of exhalent water, a plastic tube was inserted between the valves bordering the exhalent aperture. To hold this tube in position, and to ensure that all the exhalent water passed through it, the dorsal half of the mussel was embedded in plaster of Paris. The tube extended over the side of the tank containing the mussel and ended at the same level as the water level in that tank, which was kept constant. The water which dripped from the tube had therefore been pumped by the mussel, and the rate of pumping could be recorded with a measuring cylinder and stop-watch. The pumping rates observed, generally between 100 and 200 ml. per hour, were less than those of normal mussels, because of the resistance of the tube.

Pumping was frequently interrupted by periods of rest. A record for one specimen shows that every few minutes it stopped pumping for a period of about half a minute. When much suspended material was present, pumping rates were somewhat decreased.

The percentage of particles strained was estimated by comparing the light-extinction coefficients of the inhalent and exhalent water. Particles about 30 μ in diameter, such as *Lycopodium* spores and starch grains, were always 75–100% retained. This was to be expected, for such particles are nearly as large as the gill ostia. Blood corpuscles, of 7–8 μ, were sometimes all retained, sometimes partially retained, and sometimes all let through, independently of the pumping rate and apparently at will. Bacteria and flagellates 2 μ in diameter, and milk particles varying in size, but mostly much smaller than 6 μ, were never more than about 50% retained, and usually far less. With milk, the ratios between the extinction values, obtained in light of different wave-lengths, were greater for the exhalent water than for the inhalent water, showing that the exhalent water contained a higher proportion of small particles. Evidently the larger particles were retained to a greater extent than the smaller ones. It was shown that particles of a given size were filtered with about the same efficiency, whether they were mixed with particles of other sizes or not.

Colloids and particles of carbon black (less than 0·3 μ) were scarcely retained at all by the mussels. The uptake of dyes by the gills or mantle, which has been found useful experimentally as a measure of the pumping rates of mussels, does not, of course, involve straining.

Very interesting evidence as to how particles are retained was derived from microscopical observations through the transparent shells of very young mussels, placed in hanging drops of a carmine suspension. Carmine particles were caught individually by the long latero-frontal cilia, which were evidently sticky. When a cilium caught a particle, it modified its beat and wiped the particle off, on to the adjoining row of frontal cilia. It is therefore doubtful whether mussels feed by straining through a mucous layer, as MacGinitie believed, but further investigation of this point would be worth while, particularly as carmine is unpalatable to some animals. The possibility that flagellates or other food organisms induce secretion of a layer of mucus, has not been excluded. Under natural conditions, of course, mussels probably strain off flagellates and bacteria more efficiently than might appear from these experiments. Most common species of marine flagellates are considerably larger than the species used experimentally, whilst the majority of bacteria are found adhering to larger particles of silt and detritus.

E. W. Knight-Jones.

Current Bibliography

Prepared by the Editor with the Assistance of Mr. A. R. MARGETTS

NOTE. The scope of the Bibliography is limited in the main to the area covered by the Council's activities, but there are included papers of general interest relating to other areas in North Temperate and Arctic regions and exceptionally elsewhere. Limnological references are not as a rule given, but papers relating to brackish waters and to salmon and freshwater fish studied by the Council are included.

Subscribers and those on the distribution list will receive regularly upon application to the General Secretary copies of the Bibliography printed on one side of the page only, for use in making up card indices.

The Editor will be glad to be advised of emendations and additions to the Bibliography.

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Index to Volume XXI

Articles

Blackburn, M. Real and apparent changes in size of marine animal populations during the Australian fisheries investigations 284
Boerema, L. K. Some experiments on factors influencing mesh selection in trawls 175
Brandt, A. von. The efficiency of drift-nets 8
Chase, Joseph. Winds and temperatures in relation to the brood-strength of Georges Bank haddock 17
Christie, A. A., and **O. F. Newman.** The use of a magnetic stirrer in the determination of the salinity of sea water 3
Cushing, D. H. On the autumn-spawned herring races in the North Sea 44
Dannevig, Eva Henly. Chemical composition of the zones in cod otoliths 156
Fontaine, Maurice. Les océans et les dangers résultants de l'utilisation de l'énergie atomique 241
Gulland, J. A. On the selection of hake and whiting by the mesh of trawls 296
Hansen, Vagn. The food of herring on the Bløden ground (North Sea) in 1953 61
Jackman, L. A. J., and **G. A. Steven.** Temperatures and mackerel movements in the inshore waters of Torbay, Devonshire 65
Laevastu, Taivo, and **Thomas G. Thompson.** The determination and occurrence of nickel in sea water, marine organisms, and sediments 125
Lumby, J. R. A note on synoptic hydrographic charts 250
Marr, John C. The "critical period" in the early life history of marine fishes 160
Molander, Arvid R. Swedish investigations on plaice and flounder in the southern Baltic 25
Parrish, B. B., R. Jones, and **J. A. Pope.** A comparison of mesh-measuring methods 312
Qasim, S. Z. Time and duration of the spawning season in some marine teleosts in relation to their distribution 144
Truesdale, G. A., and **G. Knowles.** Some recent work on dissolved oxygen in natural waters 263
Wood, E. J. Ferguson. Fluorescent microscopy in marine microbiology 6
Wood, E. J. Ferguson. Considerations on productivity 280

Wulff, A. Über das Eingeweidefett ("Ister") des Herings 171
Wyrtki, Klaus. Monthly charts of surface salinity in Indonesian and adjacent waters 268

Reviews

Bainbridge, R. Studies on the interrelationships of zooplankton and phytoplankton (T. BRAARUD) 77
Bohl, H. Die Verbreitung der Schollenbrut in der südlichen Nordsee im März 1953 in ihrer Abhängigkeit von der hydrographischen Situation (A. C. SIMPSON) 80
Bolster, G. C. The biology and dispersal of *Mytilicola intestinalis* STEUER. A copepod parasite of mussels (P. KORRINGA) 197
Bowden, K. F. Physical oceanography of the Irish Sea (G. DIETRICH) 319
Chase, J. A comparison of certain North Atlantic wind, tide gauge and current data (O. H. SÆLEN) 192
Chipperfield, P. N. J. Observations on the breeding and settlement of *Mytilus edulis* (L.) in British waters (H. A. COLE) 334
Cloet, R. L. Sandwaves in the southern North Sea and the Persian Gulf (PH. H. KUENEN) 74
Cloet, R. L. Hydrographical analysis of the Goodwin Sands and the Brake Bank (PH. H. KUENEN) 74
Cox, Ch., and **W. Munk.** Statistics of the sea surface derived from sun glitter (J. DARBYSHIRE) 320
Demel, K., and **Z. Mulicki.** Quantitative investigations on the biological bottom productivity of the south Baltic (S. SEGERSTRÅLE) 332
Dietrich, G. Ozeanographisch-meteorologische Einflüsse auf Wasserstandsänderungen des Meeres am Beispiel der Pegelbeobachtungen von Esbjerg (E. LISITZIN) 74
Fraser, J. H. The plankton of the waters approaching the British Isles in 1953 (O. J. ØSTVEDT) 331
Fukai, R. Critical studies on the analytical methods for minor chemical constituents in sea water. I. On the estimation of phosphate-phosphorus. II. Some notes on the determination of silicate (E. FØYN) 323
Hardy, A. C., and **R. Bainbridge.** Experimental observations on the vertical migration of plankton animals (K. F. WIBORG) 78
Hela, I. The surface current field in the western part of the North Atlantic (O. H. SÆLEN) 192
Joensen, J. S. On the life history of the halibut in Faroe waters (A. D. MCINTYRE) 196
Johnston, R. Biologically active compounds in the sea (K. KALLE) . . 324
Koopman, G. Entstehung und Verbreitung von Divergenzen in der oberflächennahen Wasserbewegung der antarktischen Gewässer (G. E. R. DEACON) 75
Kändler, R. Über das Laichen der Makrele (*Scomber scombrus* L.) in der Kieler Bucht (A. J. C. JENSEN) 81

Meyer, P. Fr. Die Dampferfischerei in der Ostsee wärend der Kriegsjahre 1939/45 und ihre Bedeutung für die Fischwirtschaft und Fischereiwissenschaft (A. J. C. JENSEN) 82
Mullin, J. B., and **J. P. Riley.** The spectrophotometric determination of nitrate in natural waters, with particular reference to sea-water (E. FØYN) 323
Pollak, M. J. Static stability parameters in oceanography (J. PROUDMAN) 193
Postma, H. Hydrography of the Dutch Wadden Sea (D. J. ROCHFORD) 328
Rasmussen, B. On the geographical variation in growth and sexual development of the deep sea prawn (*Pandalus borealis* KR.) (H. A. COLE) 85
Ricker, W. R. Stock and recruitment (B. B. PARRISH) 195
Robinson, A. H. W., and **R. L. Cloet.** Coastal evolution in Sandwich Bay (PH. H. KUENEN) 74
Tammes, P. M. L., and **A. D. G. Dral.** Observations on the straining of suspensions by mussels (E. W. KNIGHT-JONES) 335
Thiel, G. Über die Auswirkung der ablenkenden Kraft der Erdumdrehung auf die Wasserstände im Grossen Belt (E. LISITZIN) 325
Werner, B. Über die Anatomie, die Entwicklung und Biologie des Veligers und der Veliconcha von *Crepidula fornicata* L. (Gastropoda Prosobranchia) (E. W. KNIGHT-JONES) 333
Wüst, G. Gesetzmässige Wechselbeziehungen zwischen Ozean und Atmosphäre in der zonalen Verteilung von Oberflächensalzgehalt, Verdunstung und Niederschlag (O. H. SÆLEN) 327
Wüst, G., W. Brogmus, und **E. Noodt.** Die Zonale Verteilung von Salzgehalt, Niederschlag, Verdunstung, Temperatur und Dichte an der Oberfläche der Ozeane (O. H. SÆLEN) 327
Wyrtki, K. Schwankungen im Wasserhaushalt der Ostsee (E. LISITZIN) 326
Østvedt, O. J. Zooplankton investigations from weather ship M in the Norwegian Sea, 1948–49 (K. M. RAE) 193

Notice and Correspondence

Obituary Notice: —
Gran, H. H. (1870–1955) by T. BRAARUD 121

Editorial Notice: —
The Current Bibliography 200

Letter to the Editor: —
B. V. Hamon (A temperature-salinity-depth recorder – Vol. XXI, 1) 72

Current Bibliography

Current Bibliography prepared by the Editor with the assistance of Mr. A. R. MARGETTS . . . . . 86, 204, and 337